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EXPERIMENTAL STUDIES OF THE JUDGMENTAL THEORY OF FEELING: IV. RETENTION OF THE EFFECTS OF LEARNED REACTIONS ON AFFECTIVE JUDGMENTS

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INTRODUCTION

The primary concern of the 3 preceding papers of this series (**28**, **29**, **30**) has been that of establishing the fact of the influence of learned reactions, specifically positive and negative reactions, upon evaluating judgments. It has been definitely determined in a series of 5 experiments that when positive reactions are learned to one set of foreign words their affective values are enhanced, and that when negative reactions are learned to another set of similar words their affective values are decreased. The significance of these results of course lies in the plausibility with which they can be generalized to the manifold of evaluating judgments made in everyday life. It is thus expedient at this point, before investigating in detail the nature and conditions of this relationship between learning and affection, to establish the plausibility of generalizing to value judgments at large.

There are 2 affinities with the affective judgments of everyday life which should obviously be demonstrated in the effects of positive and negative reactions. One is the operation of these effects for a wider variety of affective objects and for other types of approach-avoidance reactions. In the

laboratory experiments reported, Japanese words have been the objects judged and the reactions have been those of pronouncing or of not pronouncing. While in everyday life this kind of object is judged and the same kind of reactions are made, nonsymbolic affective objects, such as food, clothes, drinks, etc., are far more common and the reactions made to such objects are usually ones more easily recognizable as approach and avoidance reactions than are those of pronouncing or not pronouncing. The question of this desirable kinship will be left with mere mention for the present. The other affinity is that of the persistence of the effects of learned reactions on affective judgments. In the experiments reported so far the judgments have been taken only once after the learning situation, in which the positive and negative reactions are built up, and this one test has been taken immediately after learning. It is possible then that the measured changes in affective judgments dealt with in these experiments have been the reflections of very temporary alterations in the subjects' attitudes toward the words. In everyday life affective attitudes are relatively persistent phenomena, sometimes lasting even years, although they are obviously subject to change. The purpose of the experiment to be described in the present paper was that of investigating the influence of passage of time on the changes in affective judgments which accompany the learning of positive and negative reactions.

The present experiment, Experiment 6, is a duplicate of Experiment 5 in all respects save one, namely, the post-learning judgments were made one week after learning instead of immediately after. Forty-nine Japanese words were the objects judged, and judgments were obtained by the method of single stimuli with 2 categories. Immediately after this preliminary sorting of the words, the subject entered a learning situation in which he was presented 10 of the words on a memory drum, the problem being to respond by pronouncing whenever one of 5 appeared in the aperture and not to respond whenever one of the other 5 words appeared. The first experimental sitting was terminated as soon as the criterion

of 5 perfect trials in succession was reached. One week later the subject returned and sorted all 49 words as before.

On the basis of the natural hypothesis that changes in affective judgments wrought by learning positive and negative reactions are the same in kind as other measured effects of learning, 2 features of the results of Experiment 6 may be anticipated. For one thing, evidence of these effects is to be expected after only one week; and, secondly, the amount of the effects of learning should be less after a week than that found immediately after learning. Experiment 5 will serve as a control experiment in the demonstration of the second of these features.

Experiment 6

Sixty subjects, 47 men and 13 women, were used in this experiment. Each subject was put through the experiment in individual sittings. Since the details of procedure were, with the exception of the week interval, identical with those of Experiment 5, the reader may refer to the preceding report for them. The following important point may be reiterated here. Words symbolized by the Roman numerals I–V were the ones the subject has to learn to pronounce, and those symbolized by VI–X he had to learn not to pronounce. Only these 10 words were used in the learning problem.

The means of the 60 subjects' trials and errors in solving the learning problem, excluding the 5 criterial trials, are respectively 11.45 ± 0.93 (σ_m) and 29.42 ± 2.23. Analysis of the learning records will not be undertaken in this paper. The raw data which will be treated consist of the recordings for each subject of a P or a U for each of the 49 Japanese words both before and one week after learning. The mean number of P judgments made per subject before learning is 28.55 ± 0.69, and after learning 26.63 ± 0.90. The subjects of Experiment 6 were on the average slightly more lenient in their attitude toward the words, in both sortings, than were the subjects of Experiment 5. In Experiment 5 the corresponding mean numbers of P judgments per subject were 26.72 ± 0.47 and 25.18 ± 0.83. In both experiments the

subjects were inclined to be less critical of the words on the occasion of the second judgment of them. None of these mean differences, however, is statistically significant. The agreement between the relative standings of the 49 words on the first sorting in the 2 experiments is satisfactorily close. Pearson r between the numbers of P's given the words by the 2 groups of subjects is .944 ± .012 (P.E.$_r$) (N = 49).

The data of Experiment 6, converted into percentages of P judgments, are presented in Table I and Figure 1. These

TABLE I

PERCENTAGE OF P JUDGMENTS GIVEN EACH WORD ON THE FIRST AND SECOND SORTINGS OF EXPERIMENT 6

Words	First Sorting	Second Sorting	Words	First Sorting	Second Sorting
3	23.3	25.0	VII	61.7	60.0
I	26.7	50.0	35	61.7	53.3
7	28.3	25.0	32	63.3	60.0
1	30.0	30.0	19	63.3	51.7
4	38.3	26.7	VI	63.3	48.3
10	41.7	43.3	20	65.0	56.7
2	43.3	33.3	37	65.0	70.0
12	43.3	30.0	33	66.7	63.3
8	45.0	40.0	V	66.7	78.3
11	46.7	51.7	27	66.7	61.7
III	46.7	56.7	25	66.7	53.3
9	46.7	46.7	IX	68.3	53.3
II	48.3	63.3	16	68.3	55.0
5	48.3	35.0	28	68.3	55.0
6	50.0	55.0	VIII	68.3	43.3
24	51.7	56.7	31	70.0	66.7
13	51.7	51.7	23	71.7	71.7
14	53.3	41.7	34	71.7	53.3
15	53.3	45.0	30	71.7	65.0
22	56.7	50.0	38	71.7	68.3
IV	58.3	70.0	26	76.7	61.7
18	60.0	60.0	X	85.0	85.0
29	60.0	58.3	36	85.0	71.7
17	60.0	58.3	39	95.0	85.0
21	61.7	68.3			

correspond to a similar table and figure presented in the preceding report for Experiment 5. Table I shows the percentage of P judgments received by each of the 49 words before learning, in the second and fifth columns, and after learning, in the third and sixth columns. These figures were obtained simply by dividing each number of P judgments by 60. In the first and fourth columns of Table I are the

symbols of the words, arranged in order of increasing number of P judgments received on the first sorting. I–X stand for the words used in the learning problem, and each numeral symbolizes the same particular word here that it does in preceding reports. The Arabic numerals stand for the words 1–39, the control words, and each number symbolizes the same word in this table that it does in Table I of Experiment 5.

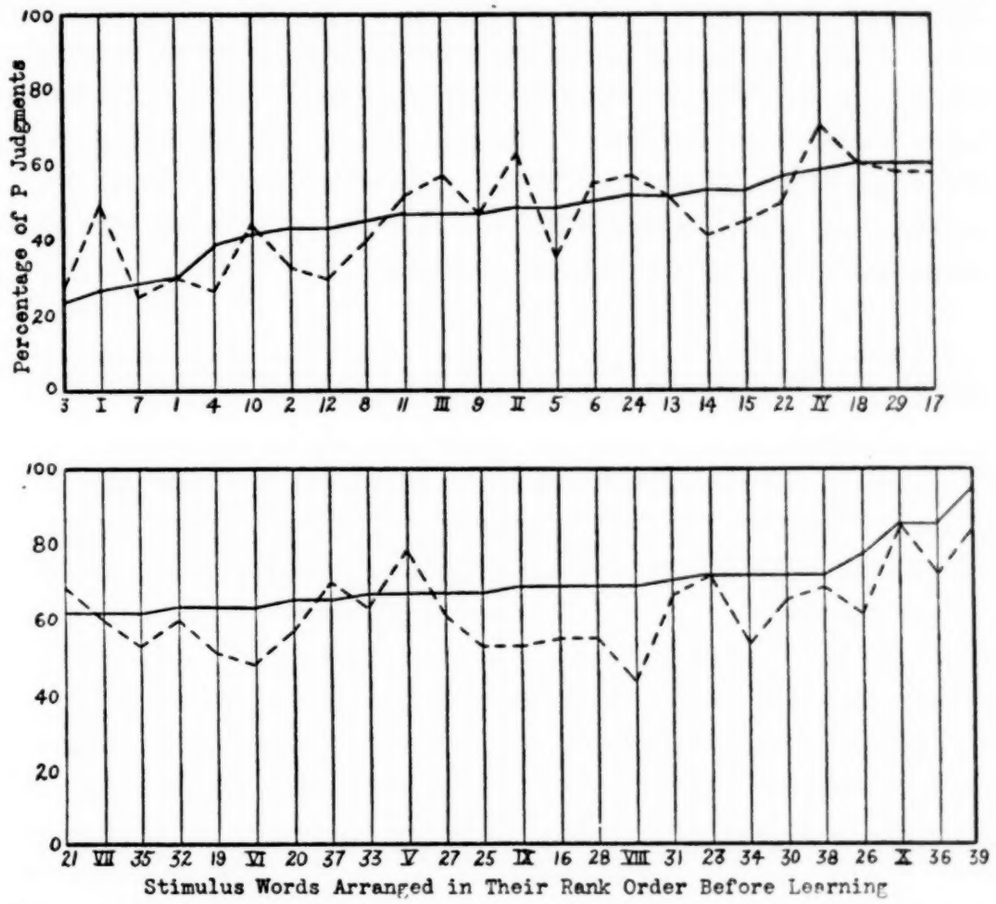
Percentage of P Judgments
100
80
60
40
20
0
3 I 7 1 4 10 2 12 8 11 III 9 II 5 6 24 13 14 15 22 IV 18 29 17
21 VII 35 32 19 VI 20 37 33 V 27 25 IX 16 28 VIII 31 23 34 30 38 26 X 36 39
Stimulus Words Arranged in Their Rank Order Before Learning

FIG. 1. The percentages of P judgments received by each word before learning (————) and after learning (— — —).

These Arabic numerals really give the rank order of 1–39 in the first sorting of Experiment 5, which was the basis of numbering them. The change in percentage of P judgments, the most useful measure in this study, is obtained for any word by subtracting the percentage on the first sorting from the percentage on the second sorting.

Figure 1 presents the data of Table I in graphic form. Along the base line the symbols of the words are arranged in

order of increasing percentage of P judgments received in the first sorting. The ordinates depict percentage of P judgments. The solid line connects the percentages before learning; the broken line connects the percentages after learning. The disparity between the 2 lines at any ordinate shows the change in percentage of P judgments for the corresponding word. Close inspection of the broken line in this figure will reveal that the 5 highest peaks above the solid line, which represent outstanding changes toward increased percentage of P's, are on the ordinates for the positive words I–V. Visual inspection of this line fails to reveal a consistent trend on the ordinates of the 5 negative words VI–X. The actual changes in percentage for the words of both sets may be found in the third column of Table II.

TABLE II

CHANGES IN PERCENTAGE OF P JUDGMENTS FOR POSITIVE AND NEGATIVE WORDS

(1) *Positive Words*

Words	Experiment 6			Experiment 5	
	Percent 1st Sorting	Percent Change (S_2-S_1)	Percent Change for Control Word	Percent 1st Sorting	Percent Change (S_2-S_1)
I	26.7	+23.3	− 0.8	38.3	+31.7
II	48.3	+15.0	−13.3	43.3	+28.4
III	46.7	+10.0	+ 2.5	43.3	+30.0
IV	58.3	+11.7	− 3.9	56.7	+25.0
V	66.7	+11.6	− 7.3	56.7	+35.0

(2) *Negative Words*

Words	Experiment 6			Experiment 5	
	Percent 1st Sorting	Percent Change (S_2-S_1)	Percent Change for Control Word	Percent 1st Sorting	Percent Change (S_2-S_1)
VI	63.3	−15.0	− 7.5	60.0	− 8.3
VII	61.7	− 1.7	− 0.9	53.3	−18.3
VIII	68.3	−25.0	−13.3	50.0	−20.0
IX	68.3	−15.0	−13.3	70.0	−21.7
X	85.0	0.0	−13.3	78.3	−16.6

The mean change in percentage of P judgments for words 1–39 is -5.30 ± 1.05. In the report of Experiment 5 the similar mean change of -5.61 ± 1.25 was explained as a

contrast effect, resulting from the increased P of words I–V. In view of the present results this simple explanation is probably inadequate, since a much smaller increase here in P of I–V is accompanied by an almost identical mean reduction in P of 1–39. This average reduction in the frequency of P judgments may reflect a shift in attitude on the part of the subjects in the direction of greater criticality as a result of having sorted the whole series of words once before, or as a result, other than the increased P of I–V, of the learning situation itself. Still another consideration, however, precludes explaining all of this reduction in frequency of P's by such a shift in attitude. It will be noticed that in Experiment 6 such a reduction in number of P judgments is to be expected largely on the basis of chance, since the majority of words on the first sorting received more than 50 percent P judgments. Such a majority of words receiving more than 50 percent P's did not occur in the first sorting of Experiment 5. Hence, the similar reduction in mean percentage obtained in Experiment 5 may have been caused, in part at least, by a contrast effect. The problem presented by this mean reduction in percentage of P judgments, the solution of which is not essential to the chief concern of this study, cannot be solved with the available data.

There is an unreliable difference between the mean changes in percentage of P judgments for words which received on the first sorting less than 50 percent P's and for words which received more than 50 percent P's. The mean change for words receiving less than 51 percent is -3.60 ± 2.06 ($\sigma_m = \sigma/\sqrt{N-2}$) (*cf.* reference 16); and the mean change for words receiving more than 50 percent is -6.06 ± 1.26 ($\sigma_m = \sigma/\sqrt{N-1}$). The numbers of cases in these 2 sets of words are respectively 12 and 27. The C. R. of the difference between the 2 means is only 1.02. But the difference is one in agreement with the difference to be expected on the basis of the operation of pure chance factors. This chance influence which varies in direction with the size of the original percentage of P judgments eliminates the simple statistical comparison of the changes for each test set of words, I–V and VI–X,

with the changes for 1–39. A reliable difference between the changes for I–V and 1–39, for example, could not be interpreted as evidence of the effects of positive responses which were connected with the former and not with the latter, because the 2 sets of words also differ in average percentage of original P judgments. In all such comparisons of changes in percentage of P judgments, the original percentages for the 2 sets of words compared must be taken into account.

In order to test first the presence of the effects of positive and negative reactions after a week, the changes in percentage of P judgments for the 2 sets of 5 test words were each compared with a set of 5 control changes extracted from the percentages for words 1–39. The 2 sets of control changes were determined by obtaining for each of the words I–X a change in percentage for a control word receiving the same original percentage as follows. In case one control word had the same original percentage, its change in percentage was taken; in case there were more than one such control word, the mean change was taken; in case no control word received the same original percentage, the mean change for words receiving just higher and just lower original percentages was used. This method of obtaining control changes is the same as that used in treatment of the data of Experiment 5. The 5 control changes for words I–V appear in the upper half of the fourth column of Table II. Each change in percentage is in the row with the test word which received a corresponding percentage of P judgments on the first sorting. The 5 control changes in percentage of P judgments for VI–X appear in the lower half of this column. Fisher's *t*-test (**9**) has been applied to the 2 series of 5 measurements for the positive words and for the negative words separately.

The mean change in percentage for the positive words is + 14.32; the mean change for 5 controls, − 4.56. The mean difference is + 18.88. When the 2 series, test and control, are treated as unrelated measurements, *t* is found to be 5.213, which for an *N* of 8 lies beyond the limits of the table of *t*. Thus P is less than .01 and the difference may be considered very significant. When the 2 series are treated as a single

series of related pairs, t is found to be 5.275. For $N = 4$ this t also lies outside the table, signifying a very significant difference.

The mean changes in percentage for the 5 negative words and their controls are respectively − 11.34 and − 9.66. The mean difference is − 1.68. Neither of the 2 methods of applying the t-test to this difference, treating the 2 series as unrelated measurements or as a single series of related pairs, shows it to be significant. By the one method $t = .3186$ and by the other, $t = .3963$. The P located in the table for both of these, each according to the appropriate N, is the same, between .8 and .7, which is not at all significant. Only the fact that the mean difference between these 2 series of measurements is in the same direction as the highly significant, and corresponding, one of Experiment 5 lends any significance to it.

In the sixth column of Table II are presented the changes in percentage of P judgments obtained in Experiment 5 for the 2 sets of test words, I–V and VI–X. Comparison for each set of test words separately of the measurements appearing in columns three and six should yield the effect of the interval of one week on the changes resulting from learned positive and negative reactions, since the week interval is the only difference between the conditions of the 2 experiments. Fisher's t-test has been applied to these sets of measurements. The method of applying the test which treats the 2 series as a single series of paired measures is eliminated in this case because, as indicated in the second and fifth columns of Table II, the test words received different percentages of P judgments on the first sortings of the 2 experiments.

The mean difference in the case of both positive and negative words between the 2 sets of 5 measurements is in agreement with the hypothesis that the effects of positive and negative reactions wear off with the passage of time. For I–V the mean difference, Experiment 5 minus Experiment 6, is + 15.70. Application of the t-test to this difference yields a t of 5.393 which, with an N of 8, is beyond the limits of the table of t. Thus the reduction in effects after a week interval

is statistically very significant. The mean difference for VI–X is − 5.64. The t-test fails to show this difference to be significant. In this case t is found to be 1.082 which, for an N of 8, locates in the table of t a P between .4 and .3. There are thus between 3 and 4 chances in 10 that this difference could have occurred by chance alone.

It will be noticed in Table II that for both sets of test words the percentages received on the first sorting tend to be larger in Experiment 6 than they are in Experiment 5. The difference is greater for VI–X. Considering the influence of the chance variable discussed above, this disparity of original percentages in the case of words I–V is unfavorable to the observed difference between changes in the 2 experiments; in the case of VI–X this disparity is favorable to the observed, but in this case unreliable, difference in changes in the 2 experiments. On the other hand, there is good reason for believing that judgments of extreme pleasantness are the most difficult to change (28, 29, 39, 40), which if true is unfavorable to the obtained difference in case of VI–X. The failure of the t-test to show significant difference between changes of VI–X in the 2 experiments should thus not be taken too seriously.

The wide variability in the changes in percentage of P judgments for words VI–X is directly responsible for failure of the t-test to show significance in both of the above compairsons *i.e.*, comparison of changes in Experiment 6 with control words of the same experiment and comparison of the same changes with those for words VI–X in Experiment 5. This variability may mean that passage of time is not accompanied by uniform changes in the influence of negative reactions. Word X actually seems to have benefited as a result of its connection in learning with a negative reaction. Although this word did not receive more P's on the second sorting than on the first, it is the only one of the words receiving the highest 4 percentages of P judgments on the first sorting that did not suffer a reduction on the second sorting. Word VII shows no effect of the negative reaction after a week. And word VIII shows a loss in percentage of P's on the second sorting of 25

which with one exception is the greatest reduction in percentage obtained for any word in either Experiment 5 or 6. This one exceptional word is 34 in Experiment 5 which changed − 26.7 on the second sorting. The change in percentage of − 15 for the remaining 2 words, VI and IX, is a relatively large one. Only one of the 39 control words shows a greater change in the same direction and one control word shows the same change.

In conclusion it may be said that Experiment 6 definitely verifies the 2 features of the results anticipated in the introduction, with respect to the effects of positive reactions on affective judgments. The effects of positive reactions are still operative after an interval of a week. And these effects after a week are not as strong as they are immediately after learning the positive reactions. While the results of Experiment 6 are inconclusive with respect to the effects of negative reactions, they slightly favor the probability that these effects are still present after a week and are then weaker than immediately after learning. On the relative strengths of the effects of positive and the effects of negative reactions, the results of Experiment 6 are in agreement with those of Experiment 5. Both sets of results show the enhancing effects of positive reactions to be stronger than the depressing effects of negative reactions.

Discussion and Review of Literature

The implications of the experiments that have been described in this series of papers for the problem of how affective judgments are developed in everyday life are very clear. According to these implications, the immediate cause of judgments of pleasantness and unpleasantness are positive and negative reactions that have in the past been made to the objects so judged. The reactions in their turn are brought about and repeatedly made through attempts of the individual to solve problems, the most constant situations of life, in which variable behavior is directed toward alleviating urges both trained and biologically inherited. Thus the child comes to appreciate Sunday school after repeatedly reacting posi-

tively to it in order to gain the affection of his mother. Or he may finally come to despise the institution after frequently reacting negatively to it in solving the problem of gaining the respect and consideration of older, more worldly, boys. Many human tastes are obviously developed in this secondary manner. Beer, for instance, is usually judged unpleasant to begin with, but in order to feel oneself to be part of a certain group he may repeatedly react positively to it and thus finally come to judge it pleasant. Many aesthetic objects, such as good paintings, are in the beginning reacted to positively by the layman in order simply to be in agreement with a recognized authority, and finally they come to be genuinely appreciated.

Knowledge of how value judgments are formed has obvious practical applications in the control of the behavior of human beings. While affective judgments are themselves brought about by the individual's reactions, they in turn act back on behavior in the form of guiding rules. In the field of social propaganda this practical importance of judgmental reactions of adults and children both is accurately estimated. On the other hand, those who constantly have the control of the behavior of children in view, parents and teachers, are usually not so clearly aware of the importance of the child's judgmental attitudes. According to the Judgmental Theory, judgmental reactions are established by stressing not the verbal statements themselves but rather the overt, adaptative behavior. One detailed practical point which is brought out by the results reported in the present series is that positive reactions are more efficacious in this matter of determining judgmental reactions, and probably more lastingly so, than are negative reactions. Thus it is more economical in the control of human affective attitudes to concentrate on devising means of arousing positive reactions to ideas, lines of action, or concrete objects, than to build up negative reactions toward other, undesirable, ideas, lines of action, or concrete objects. Thorndike (**35**, p. 75), as a result of numerous experiments, has previously arrived at the conclusion that rewards are more important than punishments in determining selection in behavior. The present experiments support a similar conclusion with respect to affective judgments.

There have been numerous isolated reports in psychological literature of experiments on the development of affective attitudes, although there have been few systematic attacks on the problem. In these studies the objects judged and the reactions aroused in the development of feelings have been more nearly like those of everyday life than are those of the present series of experiments. Emphasis in most of the studies of feeling has been placed on rewards and punishments, association, conditioning, or on sheer familiarity. Only in the present study are the positive and negative reactions stressed as the causes respectively of judgments of pleasantness and unpleasantness. In the following review of experimental studies the ease with which the typical results can be interpreted in terms of the Judgmental Theory is the chief concern of the writer. It is hoped that this will serve, for the present, in lieu of an experimental demonstration of the operation of positive and negative reactions to a wider variety of objects and reactions, a need sounded in the introduction to this paper.

The experimental studies reported by Thorndike (**35**) in the second part of his *Wants, Interests and Attitudes* constitute the nearest approach that has yet been made to a systematic attack on the genetic aspect of affective judgments. In these studies Thorndike places emphasis on the rewards or punishments that follow judgments of objects, his usually favored principle of explanation. When it is considered that the former naturally arouse responses of the approach type and that the latter arouse responses of the avoidance type, the experimental results can be easily interpreted in terms of, and thus in support of, the Judgmental Theory of feeling. These studies are of the three following types.

(1) In chapter 13 are presented a series of experiments which lead to the valuable conclusion that mere contiguity of an object with another that is like (or disliked) is not enough to cause the neutral object to be judged pleasant (or unpleasant). In order for contiguity to operate a particular affective attitude must be aroused in the subject which causes him to react to both stimuli as having something in common. The

following is an example of one of these experiments in brief. A number of neutral couplets were printed on the same sheet with some good ones and the whole set presented for judgment to subjects under 2 conditions, namely, with simply asking that each one of the whole set of poems be judged, and with special instructions which informed the subject that people of authority in literature considered all the couplets to be of merit. The first condition did not alter subjects' judgments of the neutral couplets; under the second condition the neutral poems received an excess number of P judgments and a reduced number of U judgments. The same experiment was performed with mixing neutral couplets in with bad ones, with and without corresponding authoritative suggestion. This resulted in undervaluation of the neutrals only when subjects were given the suggestion that all were bad poems. These results are easily explained by the Judgmental Theory which holds the immediate determinants of feeling judgments to be positive and negative reactions. Under the condition of mere contiguity there was nothing to arouse in the subjects a uniform type of reaction to the whole set of couplets. Thus they reacted to them individually and in a manner that varied from subject to subject with the result that their judgments showed no drift in one direction or the other. While with the condition of authoritative suggestion the subjects' reactions to the neutral couplets were with some consistency positive or negative with the result that their affective judgments showed a corresponding drift in one or the other direction. It should be noticed that the objects presented in this study were disparate ones. This experiment does not prove that in a situation which is generally disliked (or liked) by a subject, any object, or objects, to which his attention is called will not be judged in all likelihood as unpleasant (or pleasant).

(2) The second type of experiment reported by Thorndike was one in which objects containing certain fairly well defined qualities were praised to subjects and objects containing certain other qualities were disparaged. Before and after this kind of training the subjects judged sets of objects includ-

ing some of both types along with other objects of neutral qualities. One example of these studies is the following. A group of subjects first rated a set of 65 Christmas cards among which there were 21 possessing in varying degrees 3 positive qualities of landscapes, abstract designs, and gold; 21 cards possessed the negative qualities of people, ornateness, and no gold; the remaining cards were with neutral qualities. The second part of the experiment was the training period in which the subjects were required to judge a large number of Christmas cards, and after recording the judgment of each to turn the card over and read on the back a comment expressing the judgment of experts. Cards with the 3 bad qualities were disparaged; those with the good qualities were praised; and those with neutral qualities were either without comment or with a noncommittal remark. The post-training test was a repetition of the original test with another set of cards matched with the first by expert judges with respect to possession of the various qualities. Comparison of the subjects' ratings of the 2 sets of cards in the pre-training test with their ratings in the post-training test showed a definite shift toward higher rating of those with qualities praised in training and lower rating of those blamed. The results of this ingenious experiment can also be accounted for in terms of positive and negative reactions. During the training period the authoritative comments of praise and blame aroused in the subjects the respective attitudes of acceptance and rejection toward their memory idea of the immediately preceding card, in all its features. The fact that they had previously judged each card immediately prior to reading the comment on it is irrelevant to the change in judgments occasioned by the comments. Any other situation which would have centered the subjects' attention on the cards plus the expert opinions of them would have produced the same result. The setup of this training series was such as to bring about abstraction of the 2 particular kinds of qualitative attributes. The subjects reacted many times to the qualities landscape, abstract design, and presence of gold in a positive manner; and they reacted many times to the qualities people, ornateness, and absence of gold in a

negative manner. Hence, the post-training judgments showed an increase in the number of P's for objects containing the former qualities and a decrease in P's for objects containing the latter. The neutral cards showed no consistent change in judgments.

(3) Thorndike's third group of experiments are fairly typical of experimental attacks on the influence of familiarization on feeling. These need not be described separately from the others of this type to be mentioned.

The following list of experimental studies bearing on the development of affective judgments clearly shows them to be the product of training. Most of these studies are, like the ones of the present writer, experiments in changing affective judgments. Reasoning from their results to everyday life involves the assumption that if new feeling judgments can be developed in a certain way, the old ones were probably developed in the same way. The studies have been grouped on the basis of similarity of method.

One type of study on changing feeling judgments has been excluded from the list. This type includes studies of the effect of fatigue or adaptation. In these experiments an object, or number of objects, is presented for judgment in some concentrated manner, either by continuous or intermittent stimulation. It is natural to expect that judgmental reactions, like any others, should under such conditions show a temporary drift toward indifference or a state of adaptation in which reactions disappear. However, these experiments are far from showing uniformity of results. Only an oversimplified view of feelings would lead one to expect uniformity. The simple reflex, *S*—*R*, paradigm is inadequate to the treatment of affective judgments. A more appropriate, conceptual diagram would be: *S* + whole situation—positive reactions or negative reactions (present actually or symbolically)—affective judgments. Realization of the fact that the subject's personality as well as the total stimulating situation must be taken into consideration removes the puzzling aspect of the contradictions between the results of different experiments and between the varying judgmental reactions of

different subjects in the same experiment. In experiments of the adaptation type one subject who has no particular interest in either the psychological problem or in the experimenter would quickly become bored with the whole situation and, since the thing stressed in the situation is a repeated stimulus, come to react negatively to it. While on the other hand another subject who maintains interest in the whole experimental situation may become more and more interested in his introspections, and thus react in a consistently positive manner to the same stimulus.

(*A*) In the first group of studies the experiments are characterized by a method which renders them, according to the Judgmental Theory, not really studies of pleasantness and unpleasantness. The judgments of pleasantness or unpleasantness recorded in these experiments were not made by the reacting subjects, but by the experimenter who used the subjects' reactions as basis for judging the stimuli to be pleasant or unpleasant (**2**, **13**, **18**, **19**, **21**, **26**, **41**). Most of these studies have used children or infants as subjects and employed the conditioned reflex technique. The following procedure is typical. To begin with, the subject's reactions to a stimulus are recorded. Then this stimulus is presented to the subject along with another (unconditioned stimulus) which elicits a more vigorous reaction. After a few such pairings, the subject's reactions to the first stimulus alone are again observed. If, for example, the first stimulus aroused reactions interpreted by the experimenter as indifference or pleasantness, pairing it with a shock stimulus would change the child's reactions to ones interpreted by the experimenter as unpleasantness. Maslow's experiments of this type (**21**) are exceptional in that adult subjects were used and the conditioned reflex technique not used. He familiarized his subjects to one particular kind of object, such as red colored rubber bands, and surreptitiously tested the effects by offering the subjects several kinds of objects of the same class, such as rubber bands of different colors, and recorded which kind was chosen.

In so far as the studies mentioned in this first group furnish evidence bearing on methods of building up positive and negative reactions to objects, which it is assumed are the underlying determinants of affective judgment, they are significant to the psychology of pleasantness and unpleasantness. But unless an experiment includes in its data the subjects' judgmental reactions, it is not actually a study of pleasantness and unpleasantness. If this distinction is not made, practically all the animal experimentation on motivation and incentives would have to be included in the same category with studies of feeling. Pleasantness and unpleasantness are operationally undefinable in terms of any reactions other than judgments.

(*B*) A second group of experiments are essentially the same in method as the preceding ones, except in the crucial matter of who did the judging. The subjects made the affective judgments in the experiments of this second group. In 3 studies (**33**, **34**) the conditioning technique was used with children. The procedure included, first, a judgment test in which the children verbally expressed their preferences among a set of stimuli; secondly, a training period during which the subjects were individually presented with pairings of certain particular stimuli with objects calculated to arouse pleasurable emotion, such as toys or candy, the remaining stimuli of the original set being differentiated by presentation alone or with indifferent objects; lastly, the subjects were required to express again their preferences among the original set of stimuli. The results of these experiments have shown more consistent and definite changes in affective judgments than have the results of any other type of study. The results are clearly interpretable as associative shifting of positive reactions from one to another stimulus, with consequent enhancement of the pleasantness of the latter. Unfortunately in none of these experiments was the effect on pleasantness of conditioning an avoidance reaction investigated. On the basis of the results reported in the present series of studies (**28**, **29**, **30**), the prediction that conditioned negative reactions would not be

quite so thorough in changing affective judgments in the opposite direction is favored.

Two other experimental studies may be considered as belonging to this second group, although the conditioning technique was not used. Thurstone (**36**) and Peterson and Thurstone (**31**) have reported significant experiments on changing affective judgments by means of motion pictures. The likely interpretation of their results is in terms of associative shifting. In the second of these, for example, scale values were derived from children's judgments for several nationalities, including Germans, both before and after presentation of a motion picture in which Germans appeared in contiguity with character traits and behavior situations which are valued highly by the average American. The scale value for Germans relative to those for other nationalities was decidedly higher in the second than in the first scale.

(*C*) A third group of studies includes those on the influence of familiarization on affective judgments (**1, 5, 6, 7, 15, 17, 20, 21, 23, 25, 27, 35, 38, 43**). The purpose in these studies has been simply that of correlating changes in affection with repeated presentation; the behavior reactions of the subject have been disregarded. The measured change in affective judgments has been a relatively permanent one or one that extends progressively from one period of stimulation to another, as opposed to the temporary change of fatigue or adaptation. The conditions of these experiments have varied in many significant respects. The subjects have been of all ages and interests; the method of stimulation has varied widely in degree of concentration; the stimuli judged have been simple ones in some experiments, complex ones in others; and the original values of the stimuli have been P in some studies, U in others. Although the majority of these studies have yielded results favoring the conclusion that familiarization enhances affective value, it would be surprising indeed to find uniformity of results with such variation in conditions. Such uniformity would also be unexpected in view of everyday experience, where, besides the undeniable fact that people prefer the familiar and avoid the strange, evidence is easily

found for the adage that 'familiarity breeds contempt.' Familiarization, or frequent repetition, undoubtedly plays the same role here in affection that it has been found to play in learning, namely, that of a condition allowing other, determining factors to operate.

If results of individual experiments on familiarization are interpreted in the light of the probable nature of the subjects' reactions, deducible from the experimental conditions, the apparent contradictions can be resolved. In all of the experiments of this type, the subjects have been placed in a situation where they could not avoid the stimulus and have been required to make a positive reaction to it. Hence, the conclusion favored by the majority is that familiarization enhances affective value. On the other hand, should the whole experimental situation become tiresome, or even irksome, the subjects would react negatively, and 'displace' their feelings for the whole situation to the repeated stimulus object. In the experiment reported by Beebe-Center, for example, subjects were submitted to 210 presentations of a bottle of odorous substance on 6 different occasions of an hour each, and required to sniff with each presentation. The stimulus used with each subject was pleasant to begin with. It is hardly surprising that no subject's evaluation of this odor rose very much. The 3 studies (1, 27, 43) which have failed to show the enhancing effect of familiarity have employed a high degree of concentration with relatively simple stimuli, such as single colors or odors, while in the many other studies the stimuli have been complex ones, such as pictures or melodies, and the stimulation has not been so concentrated.

(*D*) The next group of studies includes the numerous investigations of preferences in different races and in children of different ages and amounts of education (**3, 4, 8, 11, 12, 14, 22, 24, 27, 32, 37, 42**). In these studies groups of subjects of different ages and races have been required to rank simple objects in order of preference. Colors, tones, melody endings, and musical intervals have been used as stimuli. While the results have not been very consistent with respect to the exact order of preferences for different groups, they show certain

significant changes in affective judgments with variations in age, education, and race. Younger children have been found to be less decided in their preferences than older ones. With increasing age and education of subjects the order of preference shows increasing closeness of agreement with the preference order of adults of the same social group. And differences in the orders of preference given a concrete set of stimuli do occur with different race groups. Since there is very little basis for an explanation of these results in terms of inherent, biological differences, the argument that they are caused by training and the pressure of cultural influences is not only evident, it is the only explanation.

(*E*) Washburn and her collaborators have performed 2 experiments on voluntarily changing affective judgments (**39, 40**). Single colors and combinations of 2 colors were used as stimuli. The procedure consisted in rating 18 stimuli on a scale of 1 to 7, and in trying immediately after the rating of each stimulus to change its affective value. The change was always attempted in a direction opposite to that of the original affective value. The resulting change, or lack of it, was recorded in terms of a second rating. Two months later each subject rated the 18 color stimuli a third time. The results showed voluntary change of affective judgments to be possible. The methods used by the subjects in effecting the change are significant in light of Thorndike's results on contiguity. In large part these methods consisted in imagining an unpleasant context for the color or combination of colors. Color combinations were found more difficult to change than single colors. Stimuli of extreme P or U value were found more difficult to change than were stimuli of intermediate values. Judgments of extremely pleasant color combinations were found to be more difficult to change than judgments of extreme U. If this last result is found to hold for other methods of changing affective judgments, it may turn out to be the underlying cause of the greater effect of positive reactions than of negative reactions found in the present writer's experiments, in which negative reactions have so far been applied only to words at the upper end of the scale.

The results of the writer's experiments are in agreement with those of Washburn in finding that changes of affective value in the direction of increasing P are more likely to persist over some period of time than are changes in the opposite direction.

(*F*) Finally, there is one experimental study that is unique in its method and bearing on the problem of changing affective judgments, and thus could not be classed with any of the above groups. In this report the writer unintentionally presented evidence favoring the hypothesis that positive and negative reactions influence pleasantness and unpleasantness. The study was reported by Fox (**10**). In his experiment a number of subjects learned 2 sonnets each; attempted to recall both with promptings from the experimenter; and finally each stated which of the sonnets was more pleasant to him. In the case of every subject it was found that the sonnet requiring fewer promptings in recall was the one preferred. Fox's problem was that of determining the effect of affective value on memory, and he concluded that the greater the pleasantness the greater the memory value. Certainly an unbiased observer would judge from the experimental setup that, since the memory test came first, it must be the causal agent. From the present writer's point of view, the following interpretation should be given this experiment. Remembering and forgetting are respectively positive and negative reactions. Therefore, the more frequently prompted sonnet received the greatest number of negative reactions and the fewest positive reactions, and accordingly was judged to be the least pleasant of the two.

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THE INFLUENCE OF THE AFFECTIVITIES UPON LEARNING *

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Introduction

The purpose of the present experiment was to determine the difference, if any, between the efficiency of learning neutral material of three types in a pleasant ('P') feeling state and in an unpleasant ('U') feeling state. The learning material was paired nonsense syllables, a series of meaningful words, and a logical passage. The feeling states were not produced by the material learned, but were induced separately just prior to the learning by means of several sensory stimuli and social conditions.

Theories of the Influence of the Affectivities upon Learning.— An influence of the affectivities upon learning is assumed by the psychoanalytic theory of repression and by Thorndike's law of effect. Psychoanalytic theory defines the influence of affect on learning in terms of conscious recall (Jones, 1915), and thus any test of active forgetting must be made after a period of retention, although the influence of the affective tone may have been present during the learning, retention, and reproduction periods. Inasmuch as the present study is concerned with immediate memory, our results do not have as direct a bearing on the principle of repression as on the law of effect. The law of effect is stated in terms of strengthening bonds between stimulus and response (Thorndike, 1932), and thus the influence must necessarily be present during learning proper, and tests of the law must be made by means of situations which include the affectivities at the time of learning.

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Previous Studies of the Influence of the Affectivities upon Learning.—The theoretical and practical importance of the present topic has stimulated many studies which have been summarized in four review articles by Douglass (1927), Meltzer (1930), Cason (1932), and Barrett (1938). All of the studies reviewed except Tait's (1913) dealt with situations in which the feeling tone was induced by the material being learned. That is to say, the learning materials themselves produced the feeling tones of the subjects. In the present study the feeling tone was induced immediately before learning the neutral materials, and this study is therefore different from practically all previous experiments.

Experiments Similar to the Present Study.—The experiment by Tait (1913) dealt with memory for lists of indifferent words, and the learning of these words was followed by the reading of other material which was P or U. Tait found a better recall for those words whose learning was followed by reading P material, although the difference was small and probably not reliable. Frank and Ludveigh (1931) also used neutral learning material. Their Ss learned nonsense syllables, after which odors ranked with respect to P and U were presented to the subjects. It was found that recall after the P odors was greater than after the U odors, there being some correspondence between recall and the degree of P. These results were interpreted as the effects of retroactive inhibition and facilitation. Inasmuch as the affective stimuli were given after learning and during retention, the procedure in these experiments was different from that used in the present study which induced feeling tone before learning.

The procedure used in the present study is similar to those experimental and life situations in which the learned material is neutral, but the total situation in which the learning takes place induces a P or U feeling tone.

Procedure

Learning Materials.—The learning material was nonsense syllables, meaningful words, and a logical passage, each of which was constructed in two forms for the two affective states. Half of the Ss took the first form in the P state, and the second form in the U; the other half having the second form in the P state and the first form in the U state. The order of the three types of materials was rotated in the six possible

ways, although for each S the order was identical in both affective states. All tests were given by the same experimenter. The instructions used in giving the tests and the experimenter's manner were as similar as possible for the P and U states.

Nonsense Syllables.—The list of 40 nonsense syllables which we used was taken from Cason's (1926, 201) study. Ten pairs of syllables were given orally in each form by the prompting method, and scored for time, errors, and repetitions. The measure of time included the complete repetition period plus the shortened interval when a correct response was given in the last repetition period. The error score was the number of times the S gave an incorrect answer, and this score was partly a function of the caution or lack of caution of the S. The repetition score was the number of times all of the pairs had to be pronounced before all correct responses were given, and this score is the most significant.

Meaningful Words.—The series of 15 meaningful words in each form were taken from Cason's (1926, 202) material. A typewritten list of these words was given to the S with instructions to read and reread the list until he was able to repeat it to the experimenter. The subject was instructed to tap after every repetition of the material, and this gave a score for number of repetitions. One error was counted every time the S failed to reproduce the whole list perfectly. As in the case of the nonsense syllables, this error was partly a function of the caution or lack of caution of the S. The time was recorded from the moment S began to read the list to the end of the successful reproduction.

Logical Passage.—The passages used were those entitled 'Cicero' and 'The Dutch Homestead' in Whipple's (1924) *Manual.* The latter passage was condensed to 125 words and 64 ideas to equalize the two passages. S silently read one of these passages for two minutes, and was then asked to write as much as he could remember. The written passage was scored for errors of logical order and of introduced material, and for 'ideas' according to Whipple's count.

Method of Inducing Affective Tone.—Each S was given one form of each of the three types of tests in both affective states, one half of the Ss being in the P state first and the U state one week later at the same hour, and the other half being in the U state first and the P state second with the same interval.

Pleasant Affective Tone.—For the P condition the S was greeted warmly by the two experimenters, and special notice was given him in the way of introductions and pleasantries. The experimental room into which he was brought was large, light and airy. If the S had taken the tests in the U state previously, joking comments were made about his reactions. Approximately 10 minutes were taken up with this kind of general conversation with two experimenters, during which an attempt was made to draw out the S on topics of interest to him academically or socially. A blank of general information was filled out during this time as the only formal part of the procedure. After this interval a short speed test was given, which consisted of writing the letters of the alphabet and serial numbers beginning with one alternately. The directions were clearly written, and an example of the method was given. The test was made to appear a part of the experiment, and the S was praised for his performance. Conversation was again continued after the test, and about 25 minutes after the S had arrived he was asked to join the experimenters in a drink of carbonated fruit juice. Five minutes later he was politely asked if he would sit at the experimental desk to take the tests. The second experimenter left at this time.

Unpleasant Affective Tone.—The S in this period was reprimanded if he were late or sat down without being told. He was always asked to wait alone for three

minutes in a small and dimly illuminated experimental room in which there was an odor of valerianic acid. Every opportunity was taken to reprimand or show anger with the S. When the S was asked at the end of six minutes to fill out an information blank, if he were to return, each mistake or question asked was criticized. Each pause in the routine was the occasion for the experimenter's staring at the wall or the subject indiscriminately, pacing the floor, tapping with his foot, reading the stop watch, and showing repugnance at the odor. At the end of 12 minutes we gave a preliminary test which in this condition consisted of confused directions to print the letters of the German alphabet backwards with consecutive odd numbers after every two letters. Noises were used to annoy the S, and the experimenter frequently bent over the S's shoulder with a watch in his hand. Any misunderstanding on the part of the S was taken advantage of, and we told the S that he was taking a long time. During the next period of obvious waiting which followed, S was given a mild solution of Epsom salts to drink. The S's reactions to the U taste were commented upon and criticized. At the end of 30 minutes S was ordered to sit at the desk and take the tests.

Affective Tone Criteria.—Just before the tests the S was asked to check how he thought the experimenters had acted toward him, and how he felt, on a 7-point scale from 'Quite P' to 'Quite U.' The S's feelings were usually of sufficient intensity to cause him to want to indicate how he felt, but little was made of this part of the procedure. S checked on a mimeographed sheet all of the emotions which he had had during the whole experimental period.

Subjects.—We used 48 Ss which were necessary for a complete rotation of conditions with the two sexes, the two affective tones, the two test forms, and the three types of material. Part or all of the procedure described above was used with 77 Ss, and of these 77 Ss, three failed to appear on the second occasion, four had to leave before the tests were completed, six were uncoöperative, six were used only in preliminary tests, and in the case of 10 Ss we did not succeed in inducing the desired affective tone. The Ss were from a large class in Elementary Psychology at the University of Wisconsin, mostly sophomores, and ranging in age from 17 to 25 years.

Results

Affective Tone.—A summary of the degrees of feeling tone checked by the Ss in this experiment is given in Table I.

TABLE I

Feeling Tone of the 48 Subjects in the Alternate P and U Affective States

Feeling Tone	Males				Females				Total
	U 1st.	P 2nd.	P 1st.	U 2nd.	U 1st.	P 2nd.	P 1st.	U 2nd.	
Quite U	1			2	2			3	8
Unpleasant	3			5	6			5	19
Mildly U	1			4	2				7
Indifferent	2			1	2			2	7
Happy	1	1	2			1			5
Mildly P	1		3			1	4		9
Pleasant	1	5	3			6	5		20
Quite P	2	6	4			4	3	2	21
Totals	12	12	12	12	12	12	12	12	96

Those Ss who checked 'Quite P' in the U procedure did so to show their irritation, as was demonstrated by their behavior and the checked list of the emotions. This is also true for those checking 'Indifferent.' In each case which might on the surface have appeared doubtful, the emotions, which the S checked on the sheet after the tests, were graded for feeling tone according to the figures for college students given by Cason (1938, 104–106); and the results of those Ss who did not have the desired feeling tone were discarded. Each S is listed twice in the table, once for the P state and once for the U. 'Happy' was included in the list of affective states by mistake, but it was checked by only five Ss.

When the U state came first, the male Ss were more reluctant than the female Ss to show their feelings in the U situation. The female Ss, on the other hand, did not respond as well as the males to the P state when this state came first. The female Ss reacted more strongly to the U state in both cases, and to the P state when it came second.

Learning Efficiency Under the Different Affective Tones.— The comparative results for the P and U states are given in Table II. A large figure indicates low learning efficiency except in the case of ideas. The first score given for each test is the best measure. Only one of the differences shown in the table is statistically reliable, the P.E.$_{diff.}$'s being less than

TABLE II

MEAN, MEDIAN, AND AVERAGE DEVIATION SCORES IN THE P AND U STATES FOR THE THREE TYPES OF LEARNING MATERIAL

Test Scores		M.			Mdn.			A. D.M.	
		P	U	Diff. P-U	P	U	Diff. P-U	P	U
Nonsense Syllables.	Repetitions	53.2± 1.8	56.7± 2.0	− 3.5± 2.8	52.0± 2.3	51.5± 2.8	0.5± 3.5	14.9	18.1
	Time	514.1±16.1	547.1±19.6	−33.0±25.3	484.0±20.1	510.0±24.5	−26.0±31.7	131.6	160.5
	Errors	14.2± 1.0	14.2± 0.8	0.0± 1.2	12.0± 1.2	12.5± 1.0	− 0.5± 1.0	7.3	6.3
Meaningful Words	Time	357.3±13.0	365.5±15.3	−8.2±21.8	340.0±16.3	350.5±19.1	−10.5±25.1	106.5	125.4
	Repetitions	20.9± 0.8	22.0± 1.0	− 1.1± 1.3	18.5± 1.0	21.0± 1.3	− 2.5± 1.6	6.5	8.4
	Errors	1.8± 0.1	1.6± 0.1	0.2± 0.1	2.0± 0.2	1.0± 0.2	1.0± 0.2	1.1	1.2
Logical Passage	Ideas	31.1± 0.9	29.0± 1.0	2.1± 1.3	31.0± 1.2	29.5± 1.2	1.5± 1.7	7.7	7.9
	Errors	1.5± 0.2	1.4± 0.1	0.1± 0.2	1.0± 0.2	1.0± 0.1	0.0± 0.2	1.3	1.0

the differences in only two cases. The one case of a reliable difference is the error score for meaningful words, but this score is not a measure of learning efficiency, and the difference is in favor of the U state. There is a consistent tendency for both the Mean and Median differences of the repetition and time scores to be lower in the P state than in the U state, and for the ideas score and errors score to be higher in the P state than in the U.

Table III gives the Mean and Median scores for the two states, and the differences between the scores for the two

TABLE III

MEAN, MEDIAN, AND AVERAGE DEVIATION SCORES IN THE P AND U STATES FOR MALE AND FEMALE Ss

Affective Tone	Test Scores	M.			Mdn.			A. D.M.	
		Male	Female	Diff. M–F	Male	Female	Diff. M–F	M	F
P	Nonsense syllables: repetitions	56.2± 3.0	50.3± 2.0	5.8± 3.6	55.5± 3.8	50.0± 2.5	5.5± 4.5	17.4	11.8
	Meaningful words: time	334.2±18.1	380.4±18.6	−46.2±25.9	325.0±22.6	352.0±23.2	−27.0±32.0	104.6	107.5
	Logical passage: ideas	30.7± 1.0	31.5± 1.7	−0.9± 1.9	35.5± 1.2	33.0± 1.2	2.5± 2.4	5.8	9.7
U	Nonsense syllables: repetitions	57.9± 2.8	55.5± 3.4	2.4± 4.4	56.5± 3.5	47.5± 4.2	9.0± 5.5	16.2	19.6
	Meaningful words: time	332.9±17.4	398.2±25.5	−65.3±30.9	337.0±21.8	380.5±31.7	−43.5±38.6	100.9	147.5
	Logical passage: ideas	29.2± 1.1	28.8± 1.6	0.4± 2.0	29.5± 1.4	29.5± 2.0	0.0± 2.4	6.4	9.5

sexes. The figures show that none of these differences is reliable.

The results given in Tables II and III indicate that affective tones induced in either male or female Ss before learning neutral material had no reliable influence upon the efficiency of learning rote material, isolated meaningful material, or logical material.

Conclusions and Discussion of Results

Induction of the Affective Tone.—Our method of inducing feeling tone which employed several sensory and social conditions, such as odor and abuse, was on the whole successful. This procedure could be used in experiments on delayed reproduction and on the influence of affective states induced during retention.

The Relation between Feeling Tone and the Learning Situation.—As the same experimenter gave the tests in both conditions, the feeling tone present in the first session may have been partially reinstated in the second session, but the verbal reports, behavior, and the checked emotions did not indicate that this was true.

While affective states were reliably induced in subjects, the subsequent set for learning may have been such that the feeling tone did not affect the learning process. The affective and learning processes may have existed simultaneously without influencing each other.

General Conclusion.—The results of this experiment show that feeling tone induced prior to learning has no influence on the efficiency of learning. This does not substantiate a theory of learning which posits greater learning efficiency when the learning is accompanied by pleasant feelings than when accompanied by unpleasant feelings. This general conclusion would apply to the law of effect and to the theory of repression insofar as this law and theory were involved in our immediate memory procedure.

Summary

1. The purpose of the present experiment was to determine the difference, if any, between the efficiency of learning paired nonsense syllables, a series of meaningful words, and a logical passage in a pleasant feeling state and in an unpleasant feeling state.

2. Practically all previous studies differ from the present one in that they used learning material which itself induced the feeling tone.

3. In the present study, both of the affective tones were induced in each of the 48 subjects; for the unpleasant state by means of petty, carping remarks to the subject, a dark room, a foul odor, a bitter drink, and a confusing preliminary test; and for the pleasant state by genial conversation with the two experimenters, a light, well-ventilated room, a cooling drink, and a simple preliminary test. The effect of these procedures was determined by verbal reports, a checked list of the emotions, and the behavior of the subject.

4. No reliable differences were found between the efficiency of learning under the pleasant state and under the unpleasant state, nor between male and female subjects in either state.

5. The results of our study are opposed to the current statements of the law of effect.

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A STUDY OF BRAIN POTENTIALS DURING MENTAL BLOCKING *

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INTRODUCTION

It has been noted many times that the curve of continuous mental work does not show the rapid and increasing decrement which is characteristic of motor fatigue curves. Arai's (**2**) experiment in multiplying four place numbers by four place numbers for twelve consecutive hours was one of the early studies. She reported approximately a 24 percent decrement in the first three hours with a 52 percent decrement for the total period. Practically no decrement was noted after the sixth hour. Thorndike's (**17**) adult subjects graded printed compositions for four consecutive hours. The work decrement for the total period was approximately three percent. Poffenberger (**16**) employed such tasks as addition and subtraction, completion of sentences, and intelligence tests, for periods of five hours. The only decrement in output appeared in the addition and subtraction and amounted to about 10 percent. Contrasted with these are records from ergograph experiments which report a complete breakdown in a few minutes.

Accepting these experimental results, Bills set about to find an explanation for them. In a long series of experiments (**5, 6, 7, 8, 9**) he developed the theory of 'mental blocks.' He defined blocks as "those periods experienced by mental workers when they seem unable to respond and cannot even by an effort continue until a short time has elapsed" (**5**). For experimental purposes he defined them statistically as reaction times which were two or more times as great as the average reaction time for a given series. His experimental situations

* A part of a thesis submitted in partial fulfillment of the requirements for the degree of Doctor of Philosophy. The writer is greatly indebted to Professor Lee Edward Travis for directing this research.

involved the performance of fairly homogeneous types of mental tasks for a continuous period of time, such as color-naming, addition and subtraction of digits, and naming opposites. Type of task and length of work period were varied systematically.

In any such work period the blocks tended to occur rhythmically and in increasing number as time progressed. Bills' theoretical discussion of these results sets forth the hypothesis that these blocks are enforced rest periods, probably neurological in character. Their presence permits the continued level of output characteristic of mental work. As further evidence that these blocks are rest periods he reported (**8**) that the introduction of artificial blocks into an experimental series 'resulted in the almost complete disappearance of the (true) blocks.' In other experiments (**7**) he demonstrated that the blocks are both central and peripheral in character.

The present study has investigated certain characteristics of the alpha brain potentials during mental blocks. Two characteristics have been studied in detail: the percentage of occurrence of the alpha rhythm, and the frequency of the alpha rhythm. The two characteristics were determined in a condition of rest, a condition of normal response, and in a condition of mental block. Comparisons were made, with particular reference to any differences between blocked responses and normal responses.

Apparatus and Procedure

The apparatus has been described in detail elsewhere (**18**). The brain potentials were recorded from the left occiput with the inactive electrode on the lobe of one ear. The subjects lay comfortably on a cot in a dark, semi-soundproofed, electrically shielded room.

Two-way communication from the soundproofed room to the recording room was maintained. Stimuli were presented verbally by the experimenter, and time of presentation recorded. The subject recorded his response by pulling a cord attached to a break-key at the moment he responded. The experimenter received the response over the communication system and presented the next stimulus immediately.

The subjects were six graduate students, all having a high percentage of occurrence of alpha waves.

All instructions were given to the subject before the experimental period began. The stimuli consisted of words to which the subject was to respond with the opposites.

He was instructed to respond as quickly as possible and maintain a high speed of performance throughout a series. Subjects were motivated to work at top speed by the initial instructions and by the experimenter's tone of voice. Most subjects increased their speed if words were presented in a hurried and clipped tone of voice. A high degree of speed was necessary to the occurrence of blocks.

The total experimental period was forty-five minutes in length. This time was divided into an initial control period of five minutes, a response period of ten minutes, another five minute control period, the second ten minute response period, etc. During the control period the subject was instructed to 'relax for five minutes.' A thirty second warning period was allowed before each series of words. Series I consisted of fifteen 'easy' opposites, such as LONG, UP, HOT, WET, EAST, etc. The words in Series II were 'hard' opposites, such as ADVANCE, DIFFICULT, CONSERVATIVE, STRENGTH, INNOCENT, STRAIGHT, etc. The words in Series III were again easy—COME, HERE, DIRTY, NEAR, SHARP, etc. In addition, a number of 'frustration words' were introduced in Series III. These were words which have no logical opposite, such as HOW, RED, NOTE, FINGER, DOG, COAT, etc. Some of these proved more difficult than others, *e.g.*, many subjects quickly responded to DOG and COAT with CAT and HAT, respectively. One frustration word was introduced in each fifteen stimulus words. Consequently, after the first fifteen responses in the series, the frustration words were the only words appearing for the first time. The total number of frustration words which any subject received depended on his speed of reaction. The highest possible total was twenty-one.

Each series of fifteen discrete words was arranged in a total list of 105 words, consisting of seven different random orders of presentation of the original series. This avoided the possibility of the order of presentation being memorized.

Definition of Terms

Alpha waves were defined as those which have a frequency between 8 and 12 per sec. The relaxation periods are referred to as control periods; the work periods as response periods; the responses during which a mental block occurs as block periods; and the periods of response to frustration words as frustration periods.

Analysis of Data

A block was arbitrarily defined as a response time at least 1.75 times the average length of response. The use of this figure, which is somewhat lower than that used by Bills, is justified by the nature of the frequency distribution of the scores. With one exception, in each series of each subject, this figure lies at least 4 probable errors above the mean. The one exception occurs in the second series of subject DS. In this distribution, 1.75 times the average length of response is 2.95 probable errors above the mean.

A response was always measured from the beginning of the stimulus signal to the beginning of the response signal. In measuring both average response time and blocks, the first sixty responses in a series were disregarded. This allowed four repetitions of each word as an initial learning period.

All response periods were recorded in terms of total time, 'in' time, and 'out' time. Division of 'in' time by total time gave the percentage of occurrence of alpha waves. Frequencies were calculated from the number of waves in each 'in' period.

Critical ratios of differences found between block and response periods, between block and control periods, and between response and control periods, were calculated. In these calculations, total number of recorded seconds was used as an N. Whether this is statistically justifiable is an open question. The critical ratios are presented for what they may be worth. Conclusions are stated only where data for all three series show the same trend.

Results and Discussion

Table I gives the percentage of occurrence of alpha waves in each control period, the period of response (beyond the learning period of sixty responses) to each series of opposites, and the block periods occurring during each series. Except in Table II, all data presented are totals for all subjects.

TABLE I

Percentage of Occurrence of Alpha Waves During Control, Response and Block Periods

	Control		Response			Blocks		
	Sec.	Percent	No. of Responses	Sec.	Percent	No. of Blocks	Sec.	Percent
Series I	1182	64.0	1749	1555	48.0	38	80	46.1
Series II	1393	64.5	1333	1724	51.2	66	242	53.4
Series III	1467	62.1	1189	1175	61.4	32	81	55.2

The percentage of occurrence of alpha waves for Series I shows 16 percent more alpha waves during control period than during response period; and 2 percent more alpha waves

TABLE II

INDIVIDUAL DIFFERENCES IN PERCENTAGE OF OCCURRENCE OF ALPHA WAVES DURING RESPONSE AND BLOCK PERIODS

	Control II	Series I		Series II		Series III	
		Response	Block	Response	Block	Response	Block
KG.....	65.9 %		*	55.7 %	64.2 %	63.7 %	53.9 %
DS.....	60.9	39.5 %	38.7 %	46.3	57.2	56.6	50.9
IC.....	49.3	20.5	8.1	33.8	14.1	40.3	23.0
HR.....	59.3	66.3	61.0	61.2	63.3	72.2	64.7
FV.....	79.7	67.0	48.1	64.7	45.1	75.1	67.6
WJ.....	73.2	45.4	9.9	45.3	31.8	50.9	27.3

* Due to errors of recording, this portion of this record could not be read.

during response period than during block period. The critical ratio (Diff./σ Diff.) of the difference between the last two percentages is .37, which is statistically insignificant. The critical ratio of the difference between control and block periods is 3.1 and is statistically significant.

The percentage differences between control and response periods will be considered in the discussion of Table V.

Data for the second control and response periods support the conclusions drawn from Series I. In this series, the direction of the difference between block and response periods is reversed, and is again insignificant. The critical ratio is .64. The critical ratio of the difference between control and block periods in this series is 3.2.

The difference between block and response periods in Series III is larger than in either of the previous series. The critical ratio of the difference is 1.1. The study of blocks in this series was unduly complicated by the introduction of the frustration words at intervals more frequent than those at which blocks would normally occur. Furthermore, blocked responses tended to occur on the word immediately following the frustration word. The percentage of occurrence is significantly lower during frustration periods than during response periods (see Table IV) and it is probable that the factors lowering the percentage of occurrence during frustra-

tion periods also lowered it during the immediately succeeding periods. This would explain the occurrence of a difference in this series, when none was present in the other two.

The critical ratio of the difference between control and block periods is 1.2, considerably lower than in Series I and II. This fact will be considered later in the paper in the discussion of adaptation.

Table I, then, indicates that there is no statistically significant difference in percentage of occurrence of alpha waves during response and block periods, though the difference between control and block periods is a true difference greater than zero.

Table II presents data on individual subjects for percentage of occurrence of alpha waves in response and block periods in each series. For purposes of comparison, percentages in the second control period are also presented. Data on individual subjects were insufficient to warrant statistical treatment.

Table III presents data for the three periods in terms of the frequency of the alpha waves which were present. In all series

TABLE III

FREQUENCY OF ALPHA WAVES DURING CONTROL, RESPONSE AND BLOCK PERIODS

	Control		Response			Blocks		
	No. Waves	Frequency	No. of Responses*	No. Waves	Frequency	No. Blocks	No. Waves	Frequency
Series I.....	1367	10.80	61	411.7	11.22	38	409	11.28
Series II.....	2378	10.65	162	1366.1	10.80	66	1363.4	10.83
Series III.....	3432	10.70	62	461.4	10.96	32	455.1	10.88

* Frequencies during control and response periods are based on selected sections of the total record.

the waves are of higher frequency in both the block and response periods than during the control period. Differences between block and response periods are small in all series and are not consistent in direction. Since the higher frequency during block periods than control periods appears in all three series and the differences are fairly large, it may be concluded,

tentatively, that there is a difference not attributable to chance.

However, this conclusion cannot be stated definitely, for the methods of analysis are inadequate. Frequencies were calculated from measurements made on the recording tape with a millimeter ruler, and such measurements are not sufficiently accurate to provide data which differentiates clearly frequency differences of less than .25 wave per second. Most of the differences in Table III are smaller than this figure.

Table IV presents data on the frustration words introduced in the third series. Percentage of occurrence of alpha

TABLE IV

COMPARISON OF FRUSTRATION, CONTROL, TOTAL RESPONSE, AND INITIAL RESPONSE PERIODS IN SERIES III

	Percentage			Frequency		
	No. Responses	Total Sec.	Percent of Occurrence	No. Responses	No. Waves	Frequency
Control		1467	62.1		3432	10.70
Response	1189	1175	61.4	288	2112	10.96
Initial response	79	125	42.5	79	573	10.86
Frustration	106	574	41.8	106	2513	10.75

waves during control period, initial responses in the series (first presentation), response, and frustration words are compared. The percentage difference between response and frustration periods is large. The critical ratio is 7.8. However, the percentage difference between the initial responses and the frustration words is negligible, the critical ratio being .14. This would indicate that the factor of newness alone, rather than frustration, could account for the difference between response and frustration periods. As would be expected, the percentage difference between control and frustration periods is large. The critical ratio is 8.4.

Frequency of the waves during the control period is lower than in any other period. All other frequency differences are small.

The intensity of feelings of frustration in this series can only be estimated qualitatively. There were large individual

differences. HR, for example, was definitely frustrated. His general tone of voice in answering the frustration words, his comments during the time while he was trying to find an appropriate answer for the frustration word, and his comments to the experimenter after the experimental period was over, all indicated a high degree of emotionality and a definite feeling of frustration. FV, on the other hand, at the introduction of the second or third frustration word, adopted the system of simply repeating the word with the prefix 'non—' or not, and when asked his reaction to these words, said he took them as a big joke. The number of times subjects asked for repetitions of frustration words and the puzzled tone of voice used in asking for a repetition, indicated that most of them felt definitely puzzled. One subject said he felt 'helpless' and 'sore at everything.' The instances cited are probably the extremes, the other subjects having more or less definite feelings of frustration.

Table V was prepared especially for the purpose of comparing control and response periods. The general findings of

TABLE V

COMPARISON OF RESPONSE AND CONTROL PERIODS

	Percentage		Frequency	
	Control	Response	Control	Response
Series I	64.0	48.0	10.80	11.22
Series II	64.5	51.2	10.65	10.80
Series III	62.1	61.4	10.70	10.96

a greater percentage of occurrence of alpha waves and a slower frequency during control than response periods are made much more apparent. The critical ratio of percentage differences in Series I is 8.4; in Series II is 7.5. However, percentages in Series III afford an exception, the critical ratio of the difference being .37. The relation of this finding to other studies in cortical adaptation will be discussed later.[1]

[1] Further evidence of adaptation appears within each series of responses. The percentage of occurrence of alpha waves during the responses to the first fifteen words in each series are respectively 34.9 percent, 38.7 percent, and 42.5 percent. The critical ratios of the difference between this and the percentage of occurrence in all responses beyond the first sixty are 2.4, 3.4, and 4.1 respectively.

The differences between control and response periods are the important positive findings of this study. As has become apparent, no significant changes in the alpha rhythm are found during a frustration period as contrasted with any initial response period, or during a block period as contrasted with a response period.

Interpretation of Results

These results may be interpreted from two viewpoints; their significance with respect to the theory of mental blocks, and their relation to research in brain potentials.

The first fact to be noted is that blocks did occur. That the alpha rhythm was not altered during their appearance may imply nothing more than that the two are entirely disparate neural phenomena. It is possible that the neural mechanism responsible for blocks, though within the central nervous system, is not a function of the cortical neurons.

Bills (**7, 8**) suggests that output in mental work is the result of the interaction of several relatively independent energy components. Each of these neural components, sensory, peripheral, and central, has its own refractory period, and in practical work situations the relation of these refractory phases to each other is important. There must be some general coördinating or facilitating process or function which maintains the balance between the various reacting arcs. If this process is impaired a block (or an error) occurs. After the slight rest allowed by the block, work proceeds smoothly again. This concept of a general coördinating process corresponds closely with the traditional concept of attention.

This process is not necessarily cortical but may be in some lower center within the central nervous system. The work of Penfield (**15**) and others indicates that the thalamus acts as a regulating and coördinating mechanism for various types of cortical activity.

Research by Adrian (**1**) and others has proved fairly conclusively that the alpha potentials are of cortical origin, but the exact physiological mechanism is as yet undiscovered; and several investigators have postulated that the regulatory

mechanism controlling them is in some sub-cortical center. Jasper (**14**), recording the alpha rhythm from several regions in the cortex, concludes that, since the various rhythms have very nearly the same frequency, they are probably controlled by "some integrating pacemaker, which may be found at a subcortical level."

Bishop (**11**) studied the brain potentials of rabbits with special reference to their relation to visual function. When the optic radiation was severed above the thalamus, he found that the rhythm disappeared. He suggests as a working hypothesis that the alpha rhythm is cyclic, the impulses traveling from the thalamus to the cortex, and back to the thalamus. Its demonstrated relation to visual phenomena suggests that it 'facilitates certain synapses of the visual pathway to sensory stimulation.'

The fact that visual stimuli are the only stimuli which are almost one hundred percent effective in reducing or obliterating the alpha rhythm would tend to corroborate Bishop's hypothesis.

If, then, the regulatory mechanisms for both mental blocks and the alpha rhythm, or for either, lie outside the cortex, this fact affords one possible explanation of the negative findings of this study.

However, there is some evidence that mental blocks are correlated with cortical phenomena which the brain potential technique is not adequately sensitive to detect. The effect of anoxemia on the two phenomena would support such a view. Bills (**10**) reported that oxygen deprivation increased the number and length of the blocks. His subjects breathed a mixture of nitrogen and air whose oxygen content varied from 8 percent to 15 percent as against the normal 21 percent. After a thirty minute period of oxygen deprivation they entered a fifteen minute work period in which they responded to four colors by pressing a different key for each. The number and length of blocks in this work period were compared to the number and length in a control period after breathing normal air, and to an equal period at the end of an hour's continuous mental work. After breathing the 15 per-

cent mixture, subjects showed no increase in number or length of blocks over the control period, but after breathing the 10.5 percent mixture they showed approximately the same increase as at the end of an hour's continuous work. In mixtures containing less than 10.5 percent oxygen, the increase in blocks was proportional to the amount of oxygen reduction.

In a study of the effects of oxygen deprivation on the brain potentials, on the other hand, Gibbs (**12**) reported that no appreciable effect was noticed until shortly before a state of unconsciousness was reached. It is possible that with finer techniques for measuring the potentials some difference would have been noted sooner. A comparison of the findings of these two studies supports the view that the presence of mental blocks is a more sensitive indicator of mental fatigue than the measurable changes in the characteristics of the alpha rhythm.

What are the implications for research in brain potentials? In discussing the present findings from this point of view, the differences between control and response periods are important. The fact that there were differences in the alpha rhythm between periods of non-activity or relaxation and active mental work indicates that the alpha rhythm is sensitive to gross differences in mental state. The greater percentage of occurrence of alpha waves during non-activity than during response period was marked, especially in the first and second control periods versus the first and second response periods.

In the third series no difference was found. However, this was due, not to a decrease in the percentage of occurrence during relaxation periods, but to the gradual increase during response periods from series to series. This increase is interesting in view of the findings of Bagchi (**3**) concerning adaptation in the alpha rhythm. Bagchi studied adaptation to a continuous tone, to an intermittent tone, to a single word repeated every second, and to a series of discrete words presented at a rate of one per second. He reported that the alpha rhythm became adapted to all these conditions. He also reported adaptation to continuous light. Interpreting

the results offered here as adaptation requires a somewhat different definition of the term than was used by Bagchi. His stimulation periods were never more than a few minutes long, and his criterion of adaptation the return of the alpha rhythm. As here used, adaptation refers to the increase in percentage of occurrence of the alpha rhythm, both within a given series, and within the total range of the experiment. Complete adaptation would be a percentage of occurrence of the alpha waves during continuous mental work which is equal to the percentage occurring during a rest period.

The frequency differences from control period to response period are consistent in direction from series to series, the alpha waves during a response period being faster than during rest. These, with the percentage differences noted above, indicate real differences in the characteristics of the alpha rhythm during a period of mental effort and one of relaxation.

No significant differences were found between mental activity during a block and during a normal response period. This would indicate that, with our present recording devices, the brain wave technique is valuable as a research instrument in the study of widely differing mental states, but is inadequate to the study of closely related types of mental activity. This conclusion is supported by another line of evidence, that offered by the studies of frustration words. They showed a significantly lower percentage of occurrence of the alpha waves than the responses immediately preceding and following them, but comparison of the frustration words with the initial words in the same series revealed that this difference may be accounted for by newness alone.

The mental activity accompanying response to the frustration words differed from that accompanying the ordinary responses in two respects: (1) the intellectual level of difficulty of the task was higher; and (2) there was an emotional reaction in the face of a task impossible of solution. The intensity of this emotional reaction has been discussed elsewhere. The combined psychological effect of the greater difficulty and the greater emotionality produced no significant effect on the alpha rhythm.

As has been suggested elsewhere in this paper, more refined techniques of analysis might reveal differences to which our present methods are insensitive. A differential analyzer developed by Grass and Gibbs (**13**) makes a fine measure of mean amplitude versus frequency which should assist greatly in the analysis of small differences in the electroencephalogram such as might be expected to attend small differences in mental activity.

Knowledge of the characteristics of the electrical activity of the human cortex must necessarily be inconclusive until the development of improved apparatus and refined experimental techniques. However, much has been discovered with the present methods, and a knowledge of their limitations is valuable.

Summary

The findings of this study may be summarized as follows:

(1) The data reveal no relation between either the frequency or the percentage of occurrence of the alpha rhythm, and the mental block.

(2) Characteristic differences in the alpha rhythm are found between periods of relaxation and periods of response to opposites-words. The percentage of occurrence of the alpha rhythm is significantly greater, and the number of waves per second probably smaller in the control period than during the response or block periods.

(3) The periods of response to frustration words (words which have no logical opposites) resemble the periods of response to the initial presentation of the non-frustration words (words which have logical opposites) in percentage of occurrence of the alpha rhythm, and show no large differences from either the normal responses or the initial responses in regard to the frequency of waves.

(4) There is evidence of adaptation of the alpha rhythm within each series of responses and during the total experimental period.

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BRAIN POTENTIALS DURING SLEEP: A COMPARATIVE STUDY OF THE DOMINANT AND NON-DOMINANT ALPHA GROUPS

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This study was undertaken to discover whether or not the great individual differences in the electroencephalographic record which have been variously reported (**2**, **3**, **5**, **8**, **12**) would be found if some condition of recording other than that now taken to be standard (room dark, eyes closed, mind at ease, body relaxed, but subject *awake*) were imposed.

The significance of such a study lies in the fact that it has been suggested that the percentage of time the alpha wave appears in the electroencephalographic record is an organismic correlate of personality (**5**), that the alpha rhythm, and other characteristics of the record, may be determined by heredity [1] (**3**) and that the alpha rhythm may reveal inborn characteristics of cortical organization (**3**).

If these individual differences in the electroencephalogram are indeed evidence of individual differences in the organization of the cortex, then differences between individuals under other than standard conditions should be expected, as well as a fair degree of correlation between the differences in the standard and the other than standard conditions. Otherwise it appears difficult to conceive of the alpha rhythm as *the* index of cortical organization.

Problem

Specifically, this investigation has been concerned with discovering whether individuals who were clearly distinguished by their alpha indices *when awake* could be as clearly distinguished, in degrees and directions of differences, on the basis of other rhythms, *when asleep*.

[1] There is some recent evidence on this point (**6**) which does not entirely support such a contention. The evidence in question, however, is admittedly limited.

Sleep was chosen as the second (the other than standard) condition for two reasons. First, psychological differences between individuals are very probably greatly reduced by a lowered level of consciousness; sleep is the most natural as well as the most easily attainable form of such a state. Any psychological determinants of the electrocortical record ought therefore to be of reduced importance. Second, Jasper and Andrews (7) have contended that certain rhythms recorded only during sleep are derived from other rhythms recorded during the waking state. Therefore, if the sleep rhythm derived from the waking alpha rhythm appears in, say, 35 percent of the sleep record of a person who shows only .4 percent alpha rhythm over his entire waking record, it would seem pertinent to ask whether this person lacked the cortical mechanisms for the production of the alpha rhythm, or whether such mechanisms were just not functioning at full capacity. The pertinence of the question might be increased by the discovery of the fact that another individual, normally showing 85 percent alpha rhythm over his waking record, also showed 35 percent 'alpha derived' rhythm over the course of his sleeping record.

Apparatus and Procedure

The technique followed in this problem was that of studying two groups of individuals, one group showing a great preponderance of alpha rhythm and the other showing very little. Thus the records were qualitatively as well as quantitatively different. Five persons served as subjects in each group. The subjects in the first group showed an alpha rhythm more than 65 percent of the time in their day-time records; those in the second group showed an alpha rhythm less than 5 percent of the time in their day-time records. Hereafter these two groups will be called the dominant alpha group and the non-dominant alpha group, respectively.

The amplifiers used have been previously described (11). All of the day records were made with a Boehme 2-A undulator, as were three of the night records. The remaining night records were made on a Grass four-element ink-writing oscillograph.

Surface electrodes were used, and the recording was done with one lead high on the left occiput, the other being on the lobe of the left ear.

The subjects slept on a cot in a semi-soundproofed, electrostatically shielded room. An inter-communicating system provided constant verbal contact between experimenter and subject. All recording equipment was in a room adjacent to the shielded room. The subjects retired at their usual time. They were instructed to respond vocally to an aperiodically-sounded 180-cycle tone. When they failed to respond it was assumed they were asleep. This tone also served as an alarm to call the subjects in the morning if they did not awaken spontaneously.

Movement was detected by means of the sensitive intercommunicating microphone hanging above the subject. Action currents appearing on the electrocortical record also served to indicate movement; these usually correlated well with the sound of movement.

During one night two dominant alpha subjects slept on separate cots and their potentials were recorded independently side by side on the oscillograph. On another night one dominant alpha and one non-dominant alpha subject were recorded simultaneously. All other subjects were recorded individually.

Three subjects in addition to the ten reported below were used for simultaneous recording from three areas (occipital, precentral and frontal) during sleep. The records from these experiments indicate that the data to be presented deal with potentials arising from the occipital region.

Analysis of Data

The analysis of such lengthy records must be essentially qualitative, at least for the present. The records we obtained were read as to the predominant type of wave form for any given minute (Fig. 1). The basic classification of Loomis,

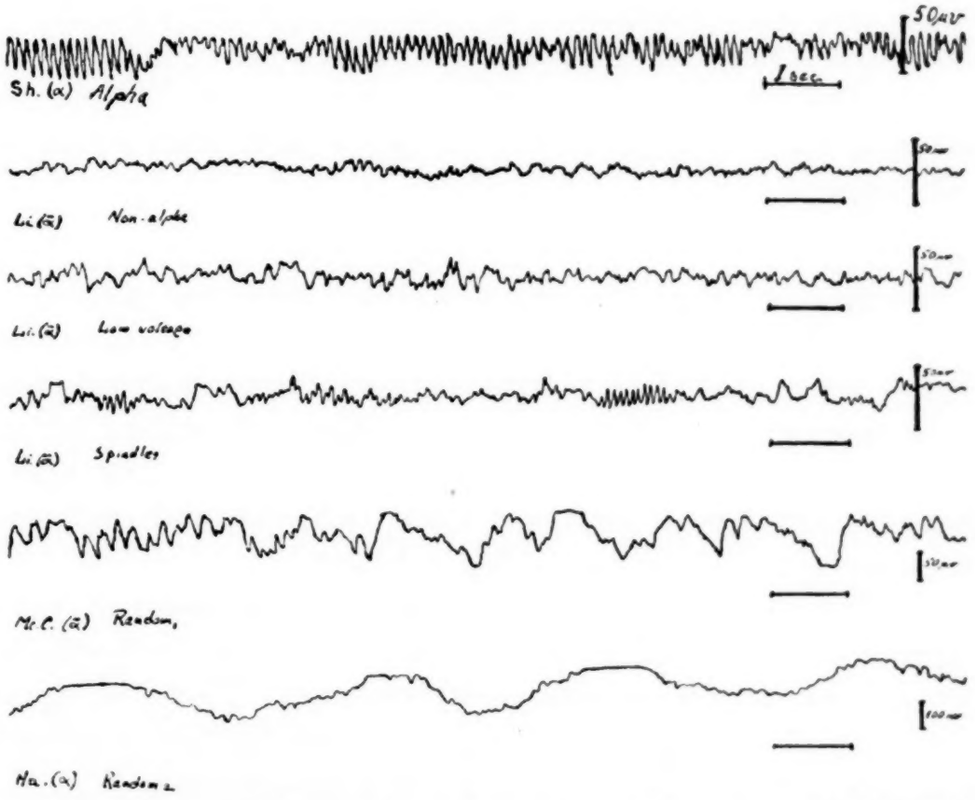

Fig. 1. Representative samples of the six 'types' of waves as classified in this report. From top to bottom: *day record*, dominant alpha subject; *day record*, non-dominant alpha subject; *low voltage; spindles; random*$_1$; *random*$_2$.

Harvey and Hobart (**10**) was modified to the following categories:

(1) *Day*, or waking, *pattern:* either dominant or non-dominant alpha.
(2) *Low voltage:* a decrease in amplitude with reference to the dominant-alpha pattern, but generally an increase in amplitude with reference to the non-dominant alpha pattern. This stage is characterized by a very irregular, non-measurable frequency.
(3) *Spindles:* a strikingly characteristic 14 per sec. wave pattern of about a second in duration.
(4) *Random*$_1$*:* high voltage (50–100 uv.), 2 to 6 per sec. waves, irregular in form and occurrence.
(5) *Random*$_2$*:* higher voltage, slow waves, lasting from one to four seconds per wave, and showing a fairly regular pattern.

We did not include the category of 'spindles plus random' which has been designated by Loomis, Harvey and Hobart because it was exceedingly difficult to locate it without benefit of selective filter bands, which we do not have. It may be assumed that we lumped this category with that which we call random$_1$.

Qualitative Results

Qualitative data are best expressed after the manner of Loomis, Harvey and Hobart (**10**). Figure 2 presents the 'running account' of the electrocortical activity of six subjects, three from each group, from the moment of retiring to the moment of awakening the following morning. The upper three records are of dominant alpha subjects, and the lower three are of non-dominant alpha subjects.

From an examination of these all-night activity charts it appears that there is little observable difference between the two groups, dominant and non-dominant. There is, apparently, as much variability of pattern among the dominant alpha subjects as there is among the non-dominant.

It is pertinent to point out that in four of the five cases in the non-dominant group there was occasionally found a 'good' alpha rhythm during the night. This was true not only during or following some bodily movement, but in one instance

before the subject went to sleep, at a time when he was very relaxed. Repeated tests on other occasions when he was

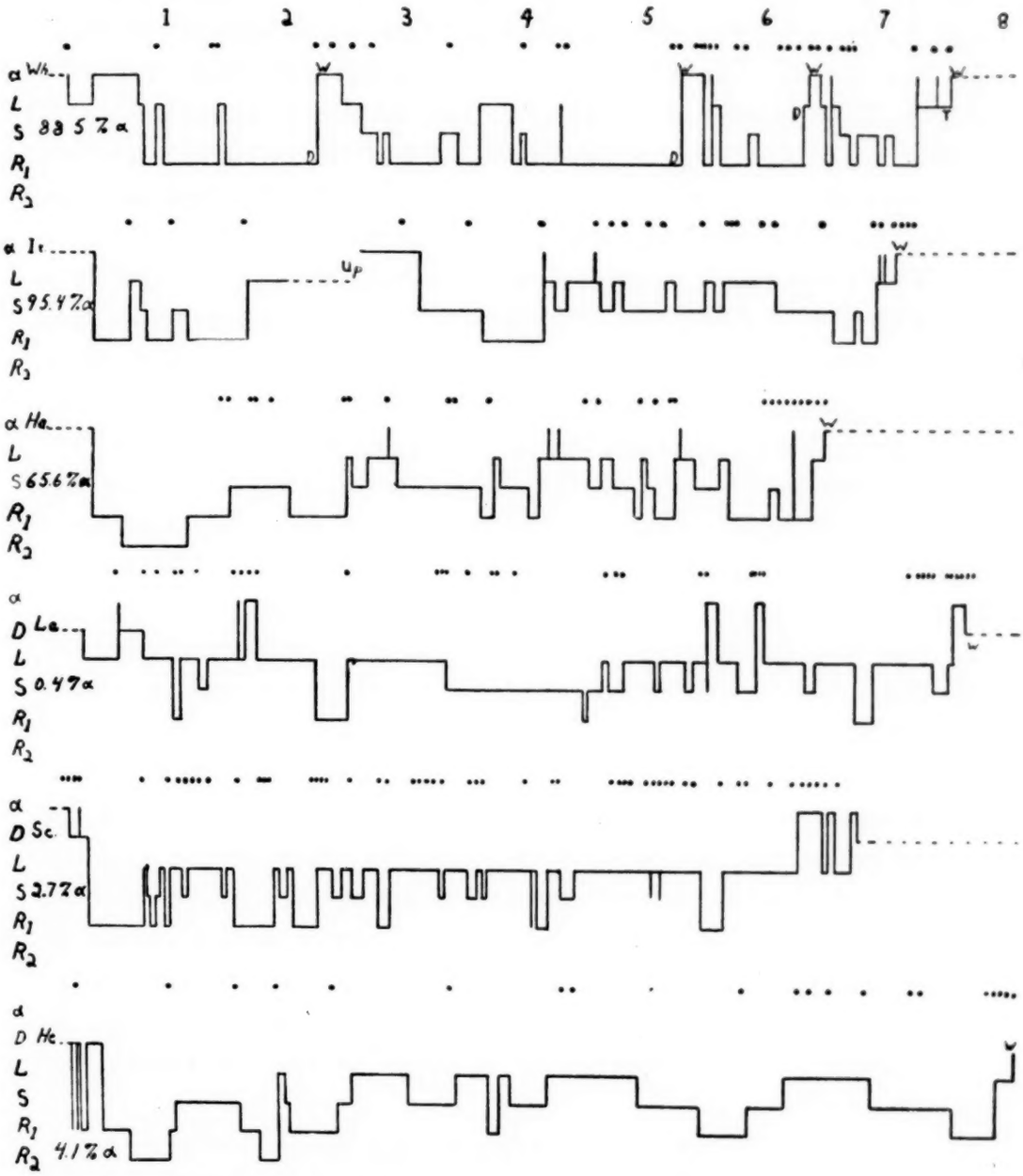

FIG. 2. All-night activity level charts for three dominant alpha, and three non-dominant alpha subjects. Letters at left indicate wave type, numbers at top indicate hours after retiring. The dots at the top of each subject's chart indicate movement. The top three charts are of dominant alpha, and the bottom three are of non-dominant alpha subjects.

awake failed to show such an abundance of the alpha rhythm as occurred at this time.

Although incidental to the purposes of this report, it might be mentioned that the clear-cur 'stair-case' effect in the plotting of all-night activity charts which has been pointed out by Loomis, Harvey and Hobart (**10**) was distinctly lacking in this study. According to our data, any stage may be omitted or displaced relative to any of the other stages; and when a subject either goes to sleep or awakens, abrupt changes in the record may occur, *i.e.*, the day pattern may be supplanted by random$_1$ waves within one minute.

The large random$_2$ waves were found here in only two subjects (one in each group). We feel that these are related to very deep sleep, since no gross movement took place while they appeared, and since these two subjects were quite familiar with the laboratory, thus being able to relax more completely.

Subjective reports of the subjects with reference to the 'quality' of rest indicated that when they felt they had slept well there was considerably less movement and less fluctuation in pattern.

Another incidental section of the data concerns dreams. All the subjects were requested, before they retired, to report any dreams experienced just prior to any spontaneous or elicited awakening during the night. One subject reported three separate dreams, two of which occurred definitely at the end of long stretches of random$_1$ patterns, the other occurring at the end of a one minute stretch of spindles. Since random waves are supposedly related to deep sleep, and spindles to lighter sleep, it appears that dreams can occur in both types of sleep. Subjects were observed to talk, cough and sigh in their sleep with no observable changes in the electrocortical patterning.

Quantitative Results

The previous section has demonstrated how very difficult it is to get any real conception of degrees and directions of differences which may exist between individuals or groups. Activity level 'running accounts' give no clear index of trends which may be present. All that one can conclude is that all the individuals studied showed nearly all types of waves during sleep, and in *apparently* comparable amounts.

In an attempt to reduce these qualitative variations to a more concise form, the percentage of the total sleep record characterized by a particular wave form has been computed. These computations were made by subtracting from the total record all those sections where it was known that the subject was awake. These immediately revealed that despite a great overlap between the two groups there were certain rather definite trends which were not evident from the qualitative examination of the data.

Tables 1, 2 and 3, and Fig. 3, present the percentage of time each type of electrocortical activity was present during

TABLE 1

MEAN PERCENTAGE OF TIME (IN MIN.) EACH WAVE TYPE APPEARED IN ALL-NIGHT SLEEP RECORDS OF DOMINANT AND NON-DOMINANT ALPHA GROUPS

	Day Alpha Index	Sleep Alpha	Low Voltage	Spindles	Random$_1$	Random$_2$
Dominant alpha group.....	82.1	5.7	31.9	23.0	37.6	1.5
Non-dominant alpha group....	1.7	3.0	43.2	29.6	20.3	1.2

TABLE 2

PERCENTAGE OF TIME (IN MIN.) EACH WAVE TYPE APPEARED IN ALL-NIGHT SLEEP RECORDS OF EACH INDIVIDUAL IN DOMINANT ALPHA GROUP

Subject	Day Alpha Index	Sleep Alpha	Low Voltage	Spindles	Random$_1$	Random$_2$
Ha.....	65.6	1.3	19.5	35.8	34.6	8.8
Tu.....	93.3	8.6	63.5	11.9	16.0	0.0
Sh.....	66.8	9.0	33.4	18.9	38.5	0.0
Wh.....	88.5	8.3	15.6	11.7	64.5	0.0
Ir.....	95.4	1.3	27.7	36.5	34.3	0.0

TABLE 3

PERCENTAGE OF TIME (IN MIN.) EACH WAVE TYPE APPEARED IN ALL-NIGHT SLEEP RECORDS OF EACH INDIVIDUAL IN NON-DOMINANT ALPHA GROUP

Subject	Day Alpha Index	Sleep Alpha	Low Voltage	Spindles	Random$_1$	Random$_2$
Sc.....	2.7	3.9	63.7	9.8	20.2	0.0
He.....	4.1	0.0	34.8	38.5	19.1	5.5
Li.....	0.4	3.4	27.9	45.6	16.8	0.0
La.....	0.4	5.1	60.4	24.0	7.6	0.0
Mc.....	0.8	2.5	29.3	30.3	37.7	0.0

sleep, both for the individuals and for the two groups. As might be expected, the mean of the dominant alpha group exceeds the mean of the non-dominant alpha group with respect to the percentage of time alpha waves were present during sleep, this difference being 2.7 percent, whereas the day-time difference was 80.4 percent. This is a great reduction (from day to night) in the amount of alpha rhythm for the dominant alpha group, but it is, interestingly enough, an

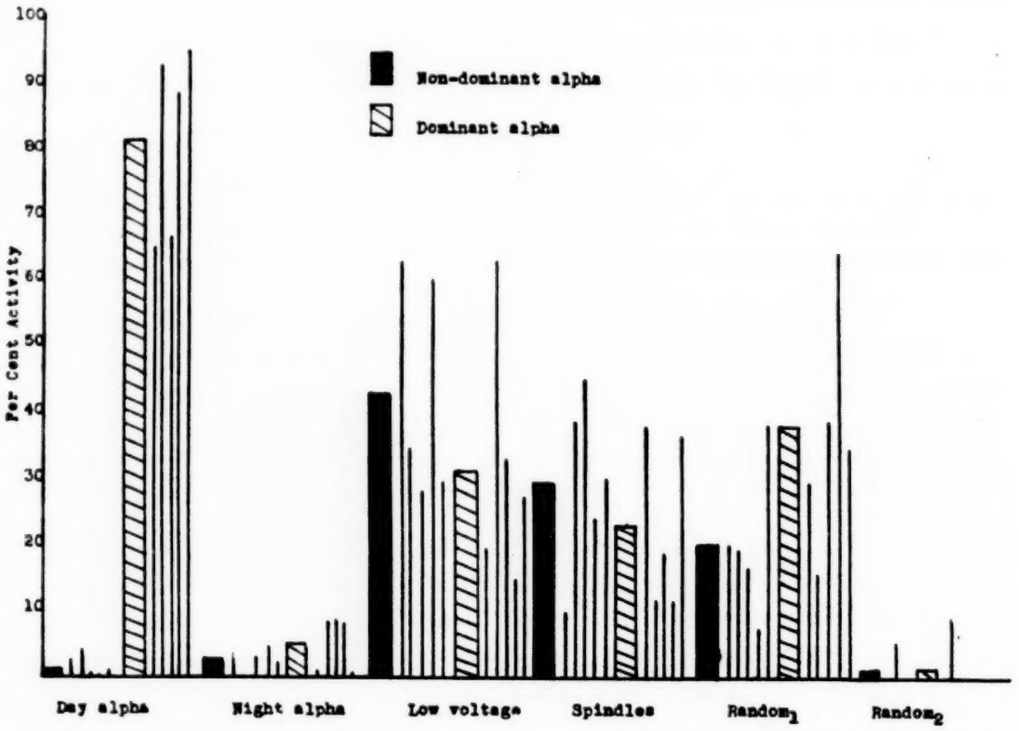

FIG. 3. Mean percent activity in each wave category during known sleep of the two groups. The single line to the right of each group mean represents the individual subjects in that group; the order is constant throughout.

increase in the amount of alpha rhythm shown by the non-dominant alpha group, as compared to its mean day-time percentage.

With respect to low voltage patterns, the trend is reversed, the non-dominant exceeding the dominant group by 11.3 percent. The degree of overlap is shown, however, by the fact that the one person with the greatest percentage of low voltage was in the dominant alpha classification.

In percentage spindles the mean of the non-dominant group is again higher than that of the dominant group, the difference

being 6.6 percent. Variations within the groups are again extreme.

In random$_1$ the difference between the two groups is the most marked, the dominant exceeding the non-dominant by 17.3 percent. In only one instance does a member of the non-dominant group equal the mean of the dominant group, and in only one instance does a member of the dominant group fall below the mean of the non-dominant group. Although this difference was the greatest observed during sleep, it is far less than the difference in the (day) alpha index; and random waves are presumably derived from the alpha rhythm.

The difference in random$_2$ can hardly be discussed, with only one case in each group showing this wave form.

In order to assist in the evaluation of the various differences, we have computed from the all-night activity level charts of Loomis, Harvey and Hobart a set of data somewhat comparable to ours. These computations were made by measuring the total length of the activity line in each wave type and dividing by the length of the line representing the total night's run. Although only two cases in each group were available, we found reasonable support of the data which we have reported.

In an attempt to establish some relationships between the percentage of time the alpha rhythm appeared on the day records of our subjects and the percentage of time each of the rhythms recorded appeared during sleep, we have computed rank order correlations for each available pair of individual percentages. These are presented in Table 4. Although these rank order correlations are not to be too highly relied

TABLE 4

RANK ORDER CORRELATIONS (rhos) BETWEEN DAY ALPHA INDEX AND PERCENTAGE OF TIME (IN MIN.) EACH SLEEP RHYTHM APPEARED ON THE RECORD, AND BETWEEN THE SLEEP RHYTHMS

	Sleep Alpha	Low Voltage	Spindles	Random$_1$
Day alpha	.15	−.26	−.24	.37
Sleep alpha		.27	−.70	.08
Low voltage			−.35	−.61
Spindles				−.24

upon, due to the small sampling and due to the wide range of talent (the result of the preliminary selection of subjects), certain trends may be used as leads for future research, and some data bearing on the question of constancy of individual differences in two different situations may be evaluated.

From these rank order correlations it may be tentatively suggested that there is no clear-cut relationship between those individual differences shown under the standard, waking, conditions of recording, and those individual differences appearing during sleep. There is need here for further research, especially with subjects who are selected over the entire frequency distribution of the alpha index, rather than with subjects selected from the extremes of the distribution, as was done here.

The other correlations, those between the various sleep rhythms themselves, are not of any particular theoretical significance, since they merely show that "the more there is of this the less there is of that," which is probably the result of depth and quality of sleep. They do serve, however, as convenient indices of relative amounts of various electrocortical activities during sleep.

The correlations between day alpha and random$_1$ and day alpha and spindles deserve separate mention, since random waves are, according to Jasper and Andrews, derived from the alpha rhythm of the waking state, and spindles are derived from the faster ('beta') rhythms of the waking state. These correlations would suggest some support for the hypothesis, since those subjects with larger alpha indices when awake tended to have more random$_1$ and fewer spindles (this last because a high alpha index means a smaller amount of faster rhythms evident on the record). Whether this would be sustained by a larger and statistically fairer sampling is problematical; research on the point is indicated.

Summary and Conclusions

1. The problem of individual differences in the electroencephalographic record (notably differences in alpha wave production) has been discussed with especial reference to the

significance of such differences in relation to a posited inborn cortical organization. It was suggested that if the differences recorded under the usual standard conditions (subject awake, room dark, eyes closed, mind at ease and body relaxed) correlated highly with differences found in other than standard conditions, the alpha rhythm and its characteristics might be considered an index of cortical organization.

2. To test this suggestion, two conditions were imposed on ten subjects. One of these conditions was the standard condition, while the second was the condition of sleep. The subjects were so selected that five had a high alpha index (over 65 percent) and five a low alpha index (less than 5 percent) in the first condition. The data from the second condition were then examined both qualitatively and quantitatively to discover whether these two groups which were so easily distinguished when awake were as easily differentiated when asleep.

3. The qualitative analysis showed individual differences in the sleep records but failed to show any differences which were obvious between the two groups.

4. The quantitative analysis showed individual differences and also showed differences between the groups, although there was some overlapping. The dominant alpha group showed 2.4 percent more alpha rhythm, 11.3 percent less low voltage, 6.6 percent less spindles, and 17.3 percent more random waves than did the non-dominant group.

5. Rank order correlations between alpha indices, computed from the standard condition, and the percentage of time each sleep rhythm appeared on the records did not indicate any high relationship between individual differences in alpha wave production when awake and individual differences when asleep.

6. Since there was greater similarity of the individual records during sleep than of the individual records during waking, it seems pertinent to inquire into the factors which have been altered.

7. One such factor is a lowering of the level of consciousness. It is possible that reduced levels of consciousness result

in more comparable cortical organizations; it is further possible that the great differences observed in the waking records are due to differences in the relationship between the person and his environment. Since these differences would be expected to regress to some (perhaps biological) mean during sleep, so might the electroencephalographic differences regress. This hypothesis is offered as a starting point for further research, not as an answer to the data.

(Manuscript received August 15, 1938)

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THE RELATIVE EFFECT OF A TIME INTERVAL UPON LEARNING AND RETENTION[1]

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Introduction

Many experiments have demonstrated the efficacy of distributed practice, that is, that the introduction of a time interval between trials facilitates learning. On the other hand, numerous experiments have demonstrated that forgetting occurs with the passage of time. Thus, a time interval has been shown to be detrimental to retention and beneficial in respect to learning. The results of these two sets of experiments seem to be contradictory, for if forgetting occurs during the interval, the paradoxical conclusion follows that forgetting is an aid to learning.

However, forgetting does not always occur with the passage of time. Under some conditions retention improves during short intervals. Such results have been reported as the phenomenon of 'reminiscence' by Ballard (**2**), Huguenin (**9**), Nicolai (**16**), Brown (**3**), Williams (**25**), G. O. McGeoch (**13**, **14**), and others. Although not all of its determining conditions are known, the fact of reminiscence has been well established.

Neither does an interval always exert a facilitative effect upon learning. Under certain conditions the introduction of a time interval between trials may be detrimental. The character of its effect is probably a function of several factors.

The length of the interval seems to be one determining condition. The insertion of extremely long intervals in the course of learning would hardly be expected to be economical. However, most of the experiments have been concerned with

[1] From the Psychological Laboratory of the University of Chicago.

The writer wishes to acknowledge her indebtedness to Professor Harvey A. Carr under whose direction this study was made.

the length of the 'optimal interval' and hence no very long intervals have been employed.

The number of times an interval is introduced during learning may also affect its efficacy. In most experiments the interval has been inserted frequently during learning, that is, after each trial or after each two or three trials. Such a frequent and repetitive introduction of the interval has operated to facilitate the rate of learning. In one study, that of McClatchy (**12**), the interval was inserted but once during learning. When a forty-eight hour interval was introduced at various stages in the learning process, it was found to be detrimental in five positions and beneficial in only one. A twenty-four hour interval also produced a detrimental effect at one point of interpolation.

Furthermore, the effect of the interval during learning has been shown to vary with the criterion used to determine its efficacy. Some experiments (**1, 7, 11, 20, 22**) have shown it to be ineffective or even detrimental to immediate recall (or speed of learning), as compared with a control condition without an interval, but superior in terms of delayed tests of retention. Obviously such results do not indicate an increased speed of learning due to the interval.

Finally, the advantages of distributed practice have sometimes merely been assumed. Some investigations (**4, 5, 6, 8, 10, 15, 17, 19, 23, 24**) have been undertaken to show *which* of several forms of distribution (or insertion of intervals in learning) was *most* economical without the inclusion of a control condition with no interval. In such studies the comparisons have been between various forms of distributed practice (or schemes of insertion of time intervals in learning). It may be noted that in some of these the most advantageous conditions have been those with the most massing of practice, that is, with fewer or shorter intervals (**18, 21**).

Although much work has been done on the effect of a time interval upon retention and upon learning, the two problems have been studied in separate experiments and hence under different experimental conditions. One differential condition

has been pointed out, that is, that the interval is usually inserted a number of times in learning experiments; whereas, only a single interval is employed in the study of retention. Inasmuch as it has been shown that an interval can exert either a beneficial or detrimental effect upon either learning or retention, it is possible that the interval may produce the same effect upon the two processes when *both* are studied under the *same* experimental conditions. Our experiment was designed to test this hypothesis.

Problem

The purpose of this investigation was to study the relative influence of a time interval upon learning and upon retention in the same experimental situation and hence under identical conditions.

Procedure

The materials used consisted of five lists of twenty-one three-letter nouns which were learned by the anticipation method with oral recall. The words were presented serially by an electrically driven drum timed for a two-second exposure for each word and a ten-second interval between successive trials. The first word in each list served as a cue for anticipation so that only twenty words were recalled. The number correctly anticipated on each trial was recorded by the experimenter, and the lists were learned to the criterion of one perfect recitation.

There were two experiments differing only in the length of the interval used. In both, the interval was inserted only once during learning. The five conditions in each experiment differed according to the stage of learning at which the interval was inserted. In Condition A the interval was introduced after the subject had recalled as many as three words on one trial. In Condition B the insertion was made after each subject had recalled at least seven words; and in Conditions C and D it was inserted after the trial on which a minimum of eleven and fifteen words, respectively, was correctly recalled. Thus, for the group as a whole, the interval was inserted after an average recall of approximately four, eight, twelve, and sixteen words. In Condition E no interval was introduced until the subject had attained the criterion set for learning, that is, one correct repetition of all twenty words.

Thirty subjects, university students, were employed in each experiment, and they were divided into five sub-groups of six subjects each according to the order in which they took the several conditions. The order of procedure is represented in Fig. 1. The five conditions are indicated by the letters A, B, C, D, and E. The sub-groups are denoted by the Roman numerals I–V, and the lists are numbered from 1 to 5 in the order in which they were learned. According to this procedure, each condition was taken an equal number of times in each position in the sequence, and each list was learned an equal number of times in each condition.

	1	2	3	4	5
I	A	B	C	D	E
II	B	C	D	E	A
III	C	D	E	A	B
IV	D	E	A	B	C
V	E	A	B	C	D

FIG. 1. Order of procedure.

Since the group results for each of the five conditions are based upon the data for all subjects in each experiment and for all five lists, the differences in group results are attributable to differences in conditions (A, B, C, D, E) rather than to differences in subjects or lists of words.

The influence of practice upon the comparative data for the five conditions was minimized in three ways: (1) All subjects learned a practice list under the regular conditions of the experiment before starting on the experimental cycle; (2) only one list was learned each week; (3) the practice effect was equated by varying the order of the five conditions in a counterbalanced fashion for the five sub-groups (as illustrated above).

The effects of two intervals was studied—one day and three days. The same lists were employed in both experiments, but of necessity two different groups of subjects were used. Each subject learned at approximately the same hour of the day and on the same days of the week throughout the experimental cycle.

RESULTS

The Effect of the Interval upon Learning.—The effect upon learning was measured by comparing the average number of trials required in each of the five conditions to learn the lists to the criterion of one perfect repetition. In Conditions A, B, C, and D the interval was inserted at different stages of learning. In Condition E no interval was employed before the criterion trial. The numerical data for each of the five conditions and for the two lengths of intervals are given in Tables 1 and 2. The mean numbers of trials to learn, the standard deviations, and standard errors of the means are given in Table 1. The differences between the number of trials to learn in Condition E and the conditions with an interval, the standard errors of the differences, and the critical ratios are presented in Table 2.

Inspection of these tables indicates that both intervals exerted a detrimental effect upon learning at all four stages at which they were introduced. On the basis of the critical ratios the detrimental effect can be considered statistically significant in five of the eight conditions. The consistency of the detrimental effect is further attested by its presence in

TABLE 1

MEAN NUMBER OF TRIALS TO LEARN

Condition	One-Day Interval			Three-Day Interval		
	Mean	S.D.	σ_m	Mean	S.D.	σ_m
A	15.80	5.00	.91	18.00	7.53	1.37
B	17.30	5.17	.94	18.53	7.11	1.30
C	17.33	5.08	.93	20.27	6.91	1.26
D	18.57	5.12	.93	19.33	7.18	1.31
E	15.23	4.12	.75	15.53	6.45	1.18

TABLE 2

DIFFERENCES IN MEAN NUMBER OF TRIALS TO LEARN WITH AND WITHOUT INTERVAL

Conditions	One-Day Interval			Three-Day Interval		
	Dif.	σdif.	C.R.	Dif.	σdif.	C.R.
A–E.....	.57	.54	1.06	2.47	1.08	2.28
B–E.....	2.07	.67	3.08	3.00	1.07	2.81
C–E.....	2.10	.63	3.34	4.74	.84	5.64
D–E.....	3.34	.65	5.15	3.80	.95	4.02

all eight cases. These results are more decisive in this respect than those of McClatchy. They suggest again that the detrimental or beneficial effect of an interval is a function of the number of times it is introduced during learning.

A greater detrimental effect was obtained for the longer interval in all four comparisons of the corresponding conditions.

For both intervals, the detrimental effect increased the later the interval was introduced with but one exception. In the case of the three-day interval the greatest number of trials was required to learn in Condition C.

The Effect of the Interval upon Retention.—Since the effect of the interval upon retention was found to be detrimental in this experiment, measures of amount of loss were calculated rather than measures of amount retained as is usually done. The loss in retention was measured in terms of: (1) the difference between the number of words recalled before and after the interval, (2) the percentage relation of this difference to the number of words recalled before the interval, (3) the number of trials required to relearn to the criterion attained

before the interval, and (4) the percentage relation of these relearning trials to the number of learning trials before the interval. The numerical data are given in Tables 3, 4, 5, and 6.

Table 3 lists the average number of words correctly recalled before and after the interval for each of the conditions

TABLE 3

MEAN NUMBER OF WORDS CORRECTLY RECALLED *

Condition	One-Day Interval		Three-Day Interval	
	Before	After	Before	After
A	4.93±.26	1.30±.27	4.60±.26	.97±.19
B	8.27±.18	2.33±.38	7.90±.21	1.57±.36
C	12.10±.21	4.57±.51	12.50±.20	2.67±.49
D	15.87±.14	6.20±.45	16.17±.20	3.93±.76
E	20.00	10.93±.74	20.00	6.03±.73

* The measures of variability are standard errors.

TABLE 4

LOSS IN RECALL AFTER THE INTERVAL

Condition	Mean Number of Words Lost		Percent of Words Lost	
	One-Day Interval	Three-Day Interval	One-Day Interval	Three-Day Interval
A	3.63	3.63	73.63	78.91
B	5.94	6.33	71.83	80.13
C	7.53	9.83	62.23	78.64
D	9.67	12.24	60.93	75.70
E	9.07	13.97	45.35	69.85

TABLE 5

MEAN NUMBER OF TRIALS TO LEARN BEFORE INTERVAL AND TO RELEARN AFTER INTERVAL *

Condition	One-Day Interval		Three-Day Interval	
	L. Trs.	Rel. Trs.	L. Trs.	Rel. Trs.
A	2.47±.13	2.33±.09	2.40±.49	2.27±.11
B	4.23±.35	2.77±.19	4.27±.53	2.93±.17
C	6.07±.39	3.57±.25	6.53±.42	5.27±.43
D	8.77±.52	4.93±.37	9.17±.64	5.77±.53
E	15.23±.75	5.57±.40	15.53±1.18	7.10±.64

* The measures of variability are standard errors.

TABLE 6

PERCENTAGE RELATION BETWEEN RELEARNING AND LEARNING TRIALS

Condition	Relearning Trs. / Learning Trs. One-Day Interval	Relearning Trs. / Learning Trs. Three-Day Interval
A	94.33	94.58
B	65.48	68.62
C	58.81	80.70
D	56.21	62.92
E	36.57	45.72

of the two experiments. The differences between these pairs of mean values and also their percentage relations to the recall scores just prior to the interval are given in Table 4. It is these values in Table 4 that are measures of the amount of loss in recall.

In columns 1 and 3 of Table 5 are listed the mean numbers of trials required to learn the lists to the criterion adopted before the interval was inserted. In columns 2 and 4 are listed the corresponding numbers of trials required to relearn the list to the same criterion after the interval. The percentage relations of these relearning trials to the corresponding learning trials in columns 1 and 3 are given in Table 6. It is these relearning trials of Table 5 and the percentage values of Table 6 that constitute the last two measures of the amount of loss due to the interval.

Two facts should be pointed out that are significant for the validity of the conclusions concerning the relative effect of the two intervals. In both experiments the interval was inserted at approximately the same stage of learning (see columns 1 and 3, Table 3), and the two groups of subjects were practically equated in learning ability as indicated by the number of trials required to learn the lists to the same stage of mastery. (Compare learning values in column 1 and 3, Table 5.)

Inspection of these tabular results indicates there was a loss in retention for each of the conditions and for both intervals according to all four measures. The amount of loss increased with the length of the interval with but two exceptions in the twenty comparisons. Both exceptions are found in Condition A which represents the lowest stage of mastery

at which the interval was inserted. In this condition the two values are alike for number of words lost (Table 4), while slightly fewer mean trials were required to relearn for the three-day interval (Table 5). These two conclusions are in harmony with all of the experimental data on retention except the 'reminiscence' experiments.

The relation of the amount of loss to the stage of mastery at which the interval was inserted varied with the method of measurement. When measured in terms of number of words lost (Table 4) and in terms of relearning trials (Table 5), the amount of loss increased the later the stage of learning at which the interval was inserted. Condition D for the one-day interval (Table 4) is the single exception. The inverse relation was obtained when the loss was measured in percentage terms. According to these measures, the amount of loss tended to decrease as the interval was inserted later in the learning process. The only exceptions occurred in the case of one reversal in percentage of words recalled (Condition B, Table 4) and another in the trials to relearn after the three-day interval (Condition C, Table 6).

This seeming variation in results with the different measures of retention is explicable when one fact is noted. Comparisons of the *percentages* of loss for the various stages at which the interval was inserted are illegitimate because these percentages are necessarily computed on different base values, and thus are functions of two variables. For example, the base values for the percentages of words lost are the numbers of words recalled prior to the interval (Table 3), and these necessarily vary with the stage of insertion. These percentages of words lost are thus functions of the two variables, number of words lost and number of words learned prior to the interval. Likewise, the base for the percentages of trials lost is the number of learning trials required to attain the selected criterion (Table 5). Since these necessarily vary with the stage of learning at which the interval was inserted, the percentage values are thus a function of two variables. For purposes of comparison of amounts of loss in retention at the various stages of learning, the only valid measures are

number of words lost (Table 4 columns 1 and 2) and number of relearning trials (Table 5). Hence, we may conclude that the amount of loss increased the later the interval was inserted.

However, the previous comparisons of percentages of loss for the one and the three-day interval at corresponding stages of learning are valid because the percentages compared are computed from approximately the same base values. (Compare columns 3 and 4 in Table 5 and the 2 columns in Table 6.) When the base values are the same, comparisons in percentage terms give exactly the same results as measures of loss in terms of number of words or of relearning trials.

The wisdom of the common practice of measuring the amount of retention in terms of percentages of recall and percentages of trials saved (saving scores) may be questioned. Comparisons of such percentage values are valid only when they are derived from the same base values, and in that case the amount retained is directly measurable in terms of amount recalled or in number of trials saved. The translation of these direct measures of retention into percentage terms gives no new measure of retention. The percentages are sometimes more convenient for purposes of comparison, but we are prone to forget that such comparisons are valid only when the same base is employed.

Retention in Relation to Efficacy of Learning.—It should be made quite clear that the use of the phrase, 'effect of a time interval upon learning and retention,' throughout the paper has been merely for sake of verbal brevity, and no implication is intended as to the causal effect of time, *per se*. The phrase should be interpreted to mean the effects that occur during an interval of time between learning trials—whatever the causal factors may be. The purpose here is not to analyze these factors but to demonstrate that they operate upon learning and retention in like manner.

Again, it should be pointed out that the purpose of this study is not to determine which method of learning (distributed or massed practice) is most desirable in terms of delayed tests of retention, but rather to study retention as it is in-

volved in the learning process. Our hypothesis was that such factors that operate to influence retention during an interval between learning trials will determine the efficacy of that method of learning.

Conclusions

When the effects of a time interval were measured by both learning and retention scores obtained from a single experimental situation, the results justify the following conclusions:

1. The introduction of the time interval exerted a detrimental effect upon both learning and retention.

2. The detrimental effect upon both learning and retention increased with the length of the time interval.

3. The degree of the detrimental effect of the time interval upon both learning and retention varied directly with the stage of learning at which it was introduced: the greater the loss in retention during the interval the greater the detrimental effect upon learning.

Our experimental results thus substantiate our hypothesis that the interval exerts the same effect upon both processes under the same experimental conditions. In the light of these data, it is unnecessary to make the paradoxical assumption that loss in retention is an aid to learning.

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THE LAW OF ACQUAINTANCE [1]

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The experiments described in the following paper were undertaken as a series of attempts to test experimentally a law of association proposed by Robinson [2] and named by him The Law of Acquaintance. He develops this law through a consideration of Ebbinghaus's discovery that nonsense syllables learned in one list could be more readily relearned even though their serial order was changed from that existing in original learning. As we all know, this led to the assumption by Ebbinghaus and others that remote associations were established between each syllable in the list and every other syllable. Robinson admits the potency of this argument but suggests that another possibility exists by which the phenomenon may also be in part explained. He writes, "Now the place-skipping theory of Ebbinghaus assumes that the fact of connection can influence the future associations of an individual, but nothing is said of the possibility that the act of reading or reciting a given syllable may, irrespective of its associative connections, influence the facility with which that act enters into new associations. . . . It is often alleged that there is a certain educational value in hearing a foreign language that one is seeking to master even if one has no idea of what the conversation is about. Is the value here to be accounted for entirely in terms of subtle associations upon which future learning can be based? I do not think so. I believe that there is a value in hearing the words, which can be advantageously abstracted from any associative connections into which they enter. If this proposition cannot be proved now, neither can the contrary one that only the factor of prior connection is really effective. Since this is the case it

[1] Research Paper No. 606, Journal Series, University of Arkansas.

[2] E. S. Robinson, *Association Theory Today*, New York: The Century Co., 1932.

is wiser to postulate a *principle of acquaintance*, because only by so doing are we likely to keep the full possibilities in mind." [4]

This statement then sets the problem for the experiments to be reported here: Are associative connections between two items more readily formed if the subjects of the experiment have had previous experience in reading and reciting these items? The experimental set-up required would seem to be simple. The subjects must first read and recite the items a given number of times. This reading must be followed by a period of formal learning of these items. The amount learned under this condition must be compared with the amount learned under a condition in which the prior reading and reciting is omitted. Five experiments have been conducted according to this general principle.

Experiment I

Experiment I was conducted as a group experiment in paired associate learning. Two groups of 47 and 42 college sophomores served as subjects. Group A was given the advantage of acquaintance with the material and hence constituted the experimental group. Group N did not have this advantage and constituted the control group.

Nonsense syllables, all taken from Glaze's [4] 13.33 percent list, were used as material. The procedure for Group A included three steps: *Step I.* A list of 14 syllables was read aloud with instructions to the subjects to copy them as they were being read. The list was read again with instructions for the subjects to check their records. The subjects were then instructed to rewrite the syllables and arrange them in alphabetical order. *Step II.* On another sheet the subjects were instructed to write pairs of syllables as the experimenter read them. The subjects were then instructed to listen to the reading again and check their records. They were then allowed four minutes to study these pairs with the instruction to learn them so that later when the first item was read by the experimenter they could write it and also the second member of each pair. The syllables making up these seven pairs were the 14 in the original list arranged in a random order. *Step III.* After the four minute study period the experimenter read off the first member of each pair with instructions to the subjects to copy it and to supply the corresponding second item in the three second interval between each item read. The serial order of the stimulus items differed from that present during the study period. All reading was done at the rate of one letter per second.

The procedure for Group N, the control group, was identical in all respects save for the omission of *Step I.* Thus Group A, given the advantage of acquaintance, has in *Step I* copied each syllable twice and read each syllable silently three, four, or more

[3] *Loc. cit.*, pp. 118–119. (Italics mine.)

[4] J. A. Glaze, The association value of non-sense syllables, *J. Genet. Psychol.*, 1928, 35, 255–267.

times prior to formal learning while this advantage has been denied Group N. There remains the simple task of determining which group has the better learning record. If the Law of Acquaintance is to be exhibited, Group A should surpass Group N. Scoring was done in terms of the number of syllables, correct as to spelling, supplied by the subjects of the two groups. These data are given in Table 1.

TABLE 1

TOTAL AND MEAN NUMBERS OF SYLLABLES SUPPLIED. ACQUAINTANCE GROUP A AND NON-ACQUAINTANCE GROUP N. EXPERIMENT I. GROUP EXPERIMENT

Group	N	Total	Mean	Median	Range
A	47	188	4.00	4	0–7
N	42	167	3.97	4	1–7

As the data of this Table show there is just no difference in the records of the two groups. The Law of Acquaintance is not supported by the results of Experiment I.

Experiment II was conducted as an individual experiment in paired associate learning. Seven pairs of nonsense syllables were used as learning material. Four conditions were arranged to provide for acquaintance (1) with all items employed in learning, (2) with the stimulus items only, (3) with the response items only, (4) with none of the items to be learned. In each condition acquaintance was introduced by requiring the subjects to spell aloud the items corresponding to each of the four conditions three times. Thus in Condition I the subject spelled aloud a list of 14 syllables later presented as 7 pairs for learning; in Condition II, the 7 stimulus items of the 7 pairs were similarly read and spelled aloud; in Condition III, the 7 response items of the 7 pairs were read and spelled aloud; in Condition IV, the subjects without previous reading of any of the items were set immediately at the task of learning the 7 pairs of syllables. The following Table 2 illustrates the procedure employed for each condition. Fourteen syllables from Glaze's 0.00 percent and 6.67 percent lists comprised the learning material for each of the four conditions. For formal learning these fourteen syllables were arranged in seven pairs for paired associate learning by the new method

TABLE 2

DESCRIPTION OF CONDITIONS FOR EXPERIMENT II

Condition	Familiarity with	Familiarity Given by
I	All syllables	Reading and spelling aloud all syllables
II	Stimulus items only	Reading and spelling aloud syllables later given as stimulus items
III	Response items only	Reading and spelling aloud syllables later given as response items
IV	No items	Subjects set immediately to the learning of the 7 pairs of syllables

developed in this laboratory.[5] Under the first three conditions of the experiment the order of presentation for learning was random with respect to the order in which the syllables had appeared for previous reading. The memory drum was set for an exposure time of 4.3 seconds for each pair or stimulus item. Records were made of the number of failures to anticipate the correct response item as each stimulus item appeared.

Sixteen subjects, with no previous experience in such experiments, were sent through all four conditions in a counter-balanced order at the rate of one condition per day. Learning was carried to one correct anticipation of all response items, *i.e.*, to one perfect presentation. The material was counter-balanced as were the subjects, that is, each group of 14 syllables appeared equally often under all four conditions.

The data obtained are given in terms of trials and errors (failures to anticipate) in Table 3. An examination of this

TABLE 3

TRIALS AND ERRORS (FAILURES TO ANTICIPATE). EXPERIMENT II. N-16

Condition	Familiar Items	Trials		Errors	
		Total	Mean	Total	Mean
I.....	All syllables	285	17.81	1252	78.25
II.....	Stim. items only	277	17.31	1231	76.94
III.....	Response items only	274	17.13	1190	74.38
IV.....	None	267	16.69	1233	77.06

Table reveals again, as in Experiment I, that the familiarity given by reading and spelling aloud prior to formal learning is ineffective in lowering records.[6]

Experiment III was a duplication of Experiment II in all respects save the fact that learning was carried only to 6 out of 7 anticipations and that all four conditions were carried through during a single sitting.[7] The data from this Experiment are given in Table 4. The trend of the data indicates a little superiority for the various acquaintance conditions. The superiority is very slight but to make sure that the differences were chance the sigmas of the various means are shown in the Table. The sizes of the sigmas are such as to

[5] L. A. Pennington and R. H. Waters, The anticipation method in paired associate learning, *J. Psychol.*, 1938, 6, 281–283.

[6] The fact that the means are so close together scarcely justifies a more refined statistical analysis for either Experiment I or Experiment II.

[7] This experiment was conducted by a student, Bernard Keating, as a part of a course in experimental psychology.

TABLE 4

TRIALS AND ERRORS (FAILURES TO ANTICIPATE). EXPERIMENT III. N-16

Condition	Familiar Items	Trials			Errors		
		Total	Mean	Sigma	Total	Mean	Sigma
I.....	All syllables	201	12.6	1.32	926	57.9	5.83
II.....	Stim. items only	210	13.1	1.13	965	60.3	4.92
III.....	Response items only	200	12.5	1.33	958	59.9	6.58
IV.....	None	218	13.6	1.31	1017	63.6	6.72

preclude the possibility that the differences between the means for the various conditions are reliable.

The three experiments thus far presented agree in showing the absence of beneficial effect from previous reading and reciting of the materials to be learned. In other words the Law of Acquaintance is given no support by the results of these three experiments.

EXPERIMENT IV

The problem in Experiment IV was to determine whether or not the Law of Acquaintance would hold for the learning of serial lists of nonsense syllables. Three conditions were employed similar in nature to those set up for the first three experiments. In Condition I the subjects first read aloud three times the list to be learned. They were then started on the formal learning of the list. The list was presented on a memory drum for learning and the serial order in this presentation was the same as that present during the reading. In Condition II the subjects read the list as above and then started the formal learning of the list. In this condition, however, the presentation order differed from that present during reading. In Condition III the subjects were started on the formal learning without previous reading of the list. The plan was that Condition III would form the control condition; that Condition I would show, in comparison with Condition III, the effect of acquaintance plus the associative connections established between the items as a result of the previous reading; that Condition II would approach a situation in which acquaintance would operate alone since presenting the list in an order different from that present during reading should tend to break up the associative connections established as a result of the reading.

Eighteen college students, naive with respect to the purpose of the experiment and without previous experience in nonsense syllable learning, were employed as subjects. The memory drum was set for a 2 second exposure rate. The subjects went through the three conditions at the rate of one per day in a counterbalanced order. Learning was carried to one perfect anticipation of the entire list.

The data, trials and errors (failures to anticipate) are given in Table 5. Statistical comparison of the data is shown in Table 6.

TABLE 5

TRIALS AND ERRORS (FAILURES TO ANTICIPATE). SERIAL LISTS. EXPERIMENT IV. N–18

Condition	Trials			Errors		
	Total	Mean	Sigma	Total	Mean	Sigma
I. Read and learn, same order	216	12.0	1.37	1005	55.8	6.93
II. Read and learn, changed order	313	17.4	1.82	1589	88.3	9.53
III. No previous reading	334	18.6	1.67	1726	95.9	9.81

TABLE 6

SIGMA DIFFERENCES AND CRITICAL RATIOS. DATA FROM TABLE 5

Conditions	Trials			Errors		
	Sig. Diff.	D	C.R.	Sig. Diff.	D	C.R.
I–III	2.18	6.6	2.03	12.01	40.1	3.34
I– II	2.69	5.4	2.01	11.78	32.5	2.76
II–III	3.72	1.2	.32	13.68	7.6	.56

These data are somewhat difficult to interpret. Condition I in which both acquaintance with the material and the associative connections set up thereby were allowed to operate satisfies the statistical requirement of significance when compared with learning in the absence of these conditions. Its statistical significance is somewhat less in comparison with Condition II, in which acquaintance was given but the operation of associative linkages interfered with by presenting the list for learning in a changed order. It still yields a critical ratio high enough to permit some prediction of a real difference in favor of learning in the same order as present during the reading. The crucial comparison for the problem under consideration is that between Conditions II and III. Here the critical ratio is far below statistical requirements. The factor of acquaintance with the material under the conditions of this experiment exerts little if any effect on the rate of learning.

It may be concluded, therefore, that presenting the lists in a changed order (Condition II) does interfere with the associative connections set up during the previous reading but

whether or not this Condition does reveal any influence of acquaintance is debatable. Under Condition II learning is at best but slightly better than under Condition III. Does that slight trend indicate the presence and operation of acquaintance, as Robinson might argue, or does it merely represent the fact that not all the associative connections were broken up, or, finally, is it merely a chance difference with no significance? The general drift of the experimental results would seem to dictate the second interpretation. If this is correct then it would have to be concluded that in Experiment IV there is no evidence for the Law of Acquaintance.

Experiment V

In all the preceding experiments the subjects knew at the beginning or at least by the second experimental sitting that they would be required to learn the materials presented them. In order to determine whether this set had any influence on the trend of the results a fifth experiment was carried out. In this experiment the subjects were first asked to come in to the laboratory for a 'speed of reading' test. For this 'test' two lists of five syllables each were prepared. The subjects in the 'speed of reading test' were asked to spell the syllables of each list aloud as quickly as possible. In an ABBA order each list was thus read five times. Time required for the reading of each list was taken by a stop watch. On the following day, as a group experiment, these same syllables were read to a large group composed of the 11 'speed of reading test' subjects and 29 control subjects. The members of the group copied the syllables as read aloud and printed on the board. Three minutes were given for study and then a recall by the method of written reproduction was taken. Records were scored in terms of the number of syllables recalled correct as to spelling. The results are shown in Table 7.

TABLE 7

'Speed of Reading Test' and Learning. Experiment V

Group	N	Syllables Recalled
'Speed'	11	8.18
Control I	16	8.50
Control II	13	8.15

Group Control I had had no previous experience either in learning nonsense syllables nor in the 'speed of reading test.' Group Control II had served in previous experiments of this series but had not been given any practice on the syllables used in this experiment. As the results clearly indicate there is no significant difference in the recall scores of any of the

three groups. None of these five attempts have resulted in demonstrating the operation of a Law of Acquaintance.

Experiment VI

The nonense syllables used in Experiments IV and V were constructed according to the following plan. In a list of 10,000 words the six most frequently used consonants are T N R S H D and the six least frequently used are V K J X Z Q.[8] In Experiment IV three lists of eight syllables each were prepared for the three conditions using these consonants and the five vowels. In each list the most frequently used consonants were placed in four syllables and the least frequently used consonants were placed in the remaining four syllables. In each list these two types of syllables were arranged in an ABBA order to overcome as far as possible the influence of primacy and finality. In each list, also, the syllables constructed were so selected as to make the average association value (taken from Glaze's list) about the same.

For the 'speed of reading test' in Experiment V syllables were similarly constructed. In one of the two lists the six most frequently used consonants were employed and in the second list the six least frequently used consonants were utilized.

This sixth experiment consisted of an examination of some of the data obtained in Experiments IV and V in order to see whether there was any relation between the ease of learning and the frequency with which the consonants appear in ordinary prose material. Thus, for Experiment IV, the question is this: Is the frequency with which the nonsense syllables of the list are anticipated related to the frequency with which the consonants of those syllables are used? The relevant data are presented in Table 8.

TABLE 8

Frequency of Consonants and Anticipation

Consonants	Anticipations			C.R.
	Mean	Sigma	Sigma Diff.	
Most used	121.60	15.70	8.87*	2.58
Least used	88.75	16.57		

* Long formula for correlated measures used. Rho is .84 between anticipations of the two lists.

The data of this table clearly indicate a relationship between the frequency with which consonants are used and the ease of anticipation of nonsense syllables in which these consonants appear. Now does this mean that the Law of Acquaintance is shown to exist? The very fact that there are most frequently used consonants must mean a greater degree of acquaintance or familiarity with those consonants. This

[8] Information supplied by Washington Service Bureau, 1013 13th St., N.W., Washington, D. C.

greater familiarity may be the factor that causes easier learning of the syllables in which they are employed. This interpretation would be in harmony with Robinson's pronouncement. But the problem is whether or not this is the correct interpretation of these data.

In a study of this question two other comparisons were made. As described above in the 'speed of reading test' of Experiment V five of the syllables making up one list contained the most frequently used consonants and the five syllables of the second list contained the least frequently used consonants. The data were taken so as to give an indication of the ease of pronounication of these two types of consonants. As has been said the two lists were read five times each in an ABBA order. The time required for each reading of each list was taken to the nearest tenth of a second. The best time of the five trials on each list made by each subject was used in figuring the means and other data shown in Table 9.

TABLE 9

TIME (SECS.) REQUIRED TO READ LISTS OF FIVE SYLLABLES. DATA FROM EXPERIMENT V

Lists	Mean	Sigma	Sigma Diff.	C.R.
Most used	2.42	.06		
Least used	2.99	.10	.12	4.83*

* Long formula for correlated measures not used since it would only increase the C.R. which is already larger than the customary 3.

The data of Table 9 show clearly that the most frequently used consonants are the more easily pronounced. It would be difficult to decide whether this were due to greater familiarity, in Robinson's sense, or to some mechanical feature of the various consonants, or to the fact of subtle associations with those consonants.

That there is some relation between the frequency of use of the various consonants and association value is shown by the second comparison made in this part of the study. This second comparison consisted in a study of the relative frequency with which these consonants appeared in Glaze's [9]

[9] *Op. cit.*

0.00 percent and 100 percent association value lists and in Witmer's[10] 0.00 percent and 4 percent and in her 92 percent, 96 percent and 100 percent lists. This comparison yielded the data of Table 10.

TABLE 10

FREQUENCY OF USE OF CONSONANTS AND ASSOCIATION VALUE

Consonants	Glaze's Lists		Witmer's Lists	
	100% List	0.00% List	0.00% and 4%	92, 96, 100%
Most used	41% (234)	10% (202)	8% (177)	56% (198)
Least used	15%	56%	66%	9%

The data are shown in terms of the percent of total consonants the six most used and the six least used consonants represent. Thus the data indicate that the six most used consonants represent 41 percent of all the consonants appearing in Glaze's 100 percent list, 10 percent of those in his 0.00 percent list and so on. The numbers in parenthesis show the total number of consonants appearing in the respective lists.

These data are enough to show that there is a high relationship between the frequency with which these consonants appear in words and the association value of syllables in which they appear. Hence any quick interpretation of the greater anticipation of syllables containing these consonants as being the result of familiarity seems unwarranted.

DISCUSSION

The results of these experiments lead one to question the theoretical validity of the concept of acquaintance as developed by Robinson. He gives no indication of the term's meaning other than that contained in the quotation given at the beginning of this paper. That statement gives no hint of the criteria or distinguishing characteristics by means of which the factor of acquaintance can be differentiated from that of remote association formation. This means that until such criteria are supplied the concept of acquaintance, while

[10] Louise R. Witmer, The association value of three-place consonant syllables. *J. Genet. Psychol.*, 1935, **47**, 337–360.

it may be a logically separable factor, cannot be experimentally isolated. As a matter of fact one may even be led to doubt its logically separate existence if one attempts an operational type of definition. This is immediately seen if one asks by what operations may acquaintance with an item be demonstrated to exist apart from the associations set up between that and other items. In other words the experimental results lead to the suspicion that acquaintance can only be defined in terms of association.

This suggestion that the Law of Acquaintance is at best a logical and not a psychological principle is not made to belittle in any way the service Robinson has rendered psychology in subjecting the association theory to the critical analysis contained in his book. By presenting this theoretical critique Robinson has opened up a wide field for experimental investigation. Though some of his contributions may be shown by this and other invesitgations to be logical rather than psychological their stimulating value remains unchanged.

Summary

To account for the readier relearning of nonsense syllables in a changed order Ebbinghaus developed the concept of remote association. Robinson has proposed as an alternate or parallel concept the principle of acquaintance. The present paper has reported six attempts to test Robinson's proposal experimentally.

In three experiments with paired associate learning conducted both as group and as individual experiments, reading and spelling the nonsense syllables prior to formal learning did not increase the speed of learning. In two experiments with serial lists, prior reading and spelling was likewise ineffective. In the sixth experiment it was shown that those syllables containing consonants most frequently used in ordinary prose were much more readily learned, could be read much more rapidly, and that these same consonants made up a large percentage of the consonants appearing in nonsense syllables of high association value. On the other hand, least frequently used consonants were more difficult to learn, less

easily read, and formed a high percentage of the consonants appearing in nonsense syllables of low association value.

The results tend to raise the question of the theoretical validity of the concept of acquaintance as a factor isolable from remote association formation. They lead to the suggestion that the acquaintance factor is at best a logical and not a psychological principle.

The final conclusion warranted by the results seems to be that of the two alternative proposals for the readier relearning of nonsense syllables the concept of remote association is to be preferred to that of acquaintance. For nonsense syllable materials the Law of Acquaintance has not been experimentally verified.

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A METHOD FOR THE DETERMINATION OF OLFACTORY THRESHOLDS IN HUMANS

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Advancement in the psychophysiology of smell is many years behind the developments in the fields of audition and vision because of the difficulties of determining accurately the terminal and differential thresholds. Various attempts have been made to devise an olfactometer which would yield precise and consistent results, but thus far these efforts have been only partially successful. The present study attempts a general criticism of many of the methods used, and describes the construction of a fairly elaborate accurate olfactometer, and illustrates its use in determining terminal thresholds.

The experimental work in the field of olfaction falls into three fairly distinct periods, distinguishable in terms of the types of apparatus employed. The earliest period was terminated by the monumental work of Zwaardemaker. A second period is characterized by the use of the Zwaardemaker olfactometer as the standard type of instrument. Within recent years various steps have been made toward devising apparatus which would afford highly accurate quantitative results.

The pioneering experimenters devised a variety of methods which, although ingenious, had no general applicability, and usually had many sources of unreliability. The work is summarized by the excellent review by Gamble in 1898.[1]

Laborious and often crude as these methods were, the quantitative results are still quoted in reputable texts such as Boring, Langfeld and Weld [2] because they are the only estimates of olfactory sensitivity now available.

[1] Gamble, E. A. McC., The applicability of Weber's Law to smell, *Amer. J. Psychol.*, 1898, 10, 82–142.

[2] Boring, E. G., Langfeld, H. S., and Weld, H. P., *Psychology*, New York, John Wiley and Sons, Inc., 1936, pp. 140–153.

Zwaardemaker's experimental work in olfaction involved a new type of olfactometer applicable to fairly general use. The theoretical formulation of the problems in olfaction and the employment of his instrument in quantitative measurements inaugurated a new era in the study of olfaction.

The Zwaardemaker olfactometer, or modifications of it, has been used by a number of different experimenters.[3] It consists of a graduated tube over which is fitted a second tube, the inner surface of which is covered with the excitatory substance. The amount of exposed surface of the outer tube is taken as an index of the amount of stimulus. Unfortunately, this apparatus is open to criticism on theoretical as well as on experimental grounds.

The chief fault with this type of apparatus comes from the fact that it does not take temperature and humidity into account. Vaporization occurs when highly energized molecules break away from the surface of a liquid or solid. When this occurs in a limited space, the air above the substance becomes saturated with the vapor thus formed. As vaporization proceeds the pressure exerted by the vapor increases. In time, however, an equilibrium is reached at which the rate of vaporization just equals the rate of condensation. The pressure exerted by the substance at this stage of equilibrium is said to be the vapor pressure of that substance. A rise in temperature has the effect of speeding up vaporization because the kinetic energy of the molecules is directly proportional to the absolute temperature. The important factor determining vapor pressure is not the amount of liquid or vapor that is exposed, as in the Zwaardemaker olfactometer, but is rather the volatile nature of the substance in relation to its temperature. Thus, the major difficulty in the use of Zwaardemaker's olfactometer arises from the assumption that

[3] Gamble, E. A. McC., The applicability of Weber's Law to smell, *Amer. J. Psychol.*, 1898, **10**, 82–142. Glaze, J. A., Sensitivity to odors and other phenomena during a fast, *Amer. J. Psychol.*, 1928, **40**, 569–575. Titchener, E. B., *Experimental Psychology:* Instructor's Manual, Qualitative, New York, MacMillan, 1901, pp. 112–142. Zwaardemaker, H., *L'odorat*, Paris, 1925. Zigler, M. J., and Holway, A. H., Differential sensitivity as determined by the amount of olfactory substance, *J. Gen. Psychol.*, 1935, **12**, 372–382.

the amount of surface exposed is a measure of an olfactory stimulus. This can be true only if a known amount of odorous material is completely evaporated into a measured space. Even though this be accomplished, and it rarely has, the experimenter now confronts the problem of the dilution of the gas by the addition of fresh air as it is inhaled from the container.

Zwaardemaker's concept of 'olfacties' as representing the amount of exposed surface which is perceptible, is fruitless for the physical determination of limens. It is based upon the notion that the current of air, in passing over odorous surfaces, becomes more saturated in direct proportion to the amount of exposed surface. But air is said to be saturated only when it has acquired vapor from a liquid to the extent that the partial pressure of the vapor in the gas mixture equals the vapor pressure of the liquid at its existing temperature. No matter how extensive the exposure at that temperature, the vapor can not become more saturated, because condensation will immediately take place.

To put the above criticism to a test, limens for three individuals were obtained using the standard type of Zwaardemaker olfactometer with red India rubber. The odorous cylinder was then cooled and each subject was tested again for his olfactory threshold. Finally, similar tests were conducted after the cylinder had been heated in a bath of boiling water. The cylinder was protected from the water each time it was heated by means of a test-tube which fitted snugly and which could be removed just before inhalation. The data are summarized as follows:

TABLE I

Subject	Limen at 22°C.	P.E.	Limen at 100°C.	Limen at −10°C.
			Not determinable	Not determinable
R. G...	2.73 units	.75	Less than .5 units	Greater than 18 units
G. K...	3.09 units	.71	Less than .5 units	Greater than 18 units
T. R...	2.81 units	.90	Less than .5 units	Greater than 18 units

These data furnish clear proof of the importance of temperature. The three subjects, whose limens were around 3

units, and with P. E. less than .9 units, could easily detect the odor when only .5 units of surface was exposed if the odorous material had been heated. When it was cooled to about − 10° C., the odorous material was not smelled even after 18 units (the limit on this olfactometer) were exposed.

The Zwaardemaker olfactometer is defective in that the molecular concentration of the vapor is unknown and the method of inhalation permits of considerable error due to uncontrolled dilution; an olfactie is a unit which is applicable only for a special apparatus under special conditions, and hence has only relative significance.

As a consequence of these experimental and theoretical difficulties, the quantitative work in olfaction using the methods so far discussed has very questionable significance. Perhaps certain conclusions with regard to the relative sensitivity to various substances may stand; but the results with regard to the absolute concentrations of stimuli have little justification.

Several ingenious devices have been developed in recent years which have been employed to give satisfactory results for some comparative purposes, but which fail to provide a device for measuring the actual concentration of the olfactory substance. The most nearly successful study, for our purposes, is a study by Woodrow and Karpman [4] who describe an olfactometer based on Avogadro's principle that equal volumes of gases at the same temperature and pressure contain an equal number of molecules. A steady stream of air from an air pump was purified by being bubbled through potassium permanganate, dried by being passed through calcium chloride, bubbled through an odorous liquid, warmed in a hot water bath, and finally passed through a nosepiece to the subject's nostrils. The odorous material was contained in an Emmerling absorption tube which was immersed in a tank of water kept at a constant temperature. Knowing the vapor pressures and the temperatures at which the various substances were volatilized, they were able to control the relative degree of concentration of the stimuli used. However, this method

[4] Woodrow, H., and Karpman, B., A new olfactometric technique, *J. Exper. Psychol.*, 1917, 2, 431–447.

was designed for use only in the study of adaptation effects and is not practical for the investigation of terminal or differential thresholds, since the exact concentrations could not be determined.

Other similar methods have recently been employed, as those by Allison and Katz, Fair, and Elsberg and his associates and others.[5] These are all subject to considerable error, such as employing rubber tubing, failing to control temperature and humidity, employing measures of the amount of the odorous material that are not accurate for threshold measurements.

There are several requirements which must be met in the construction of apparatus designed to do quantitative work in olfaction.

1. The air which is used must be pure and free from any odor.

2. The humidity and temperature must be controlled.

3. The intensity of the odorous vapor must be controlled and must be measurable in terms which are common to all science.

4. The olfactometer should not have anything in its construction which is itself odorous, and it must be so constructed that thorough cleaning at frequent intervals is possible.

The apparatus now to be described satisfies most of these requirements.

A twenty-gallon glass carboy (see Diagram 1) was fitted with a three-hole stopper. Water from the laboratory tap was forced into the vessel through a glass tube (*w*). The displaced air was forced into an airline. To keep the pressure in the line constant an escape valve consisting of a T-tube was used, the long end of which extended 10 inches into a jar of water (*P*). This limited the pressure to a certain measurable amount and prevented any sudden fluctuations.

[5] Allison, V. C., and Katz, S. H., An investigation of stenches and odors for industrial purposes, *J. Indus. Eng. Chem.*, 1919, **11**, 336–339. Elsberg, C. A., and Brewer, E. D., The sense of smell. X. A detailed description of the technique of two olfactory tests used for the localization of supratentorial tumors of the brain, *Bull. Neurol. Inst., N. Y.*, 1935, **4**, 501–510. Report of the Committee on control of tastes and odors in public water supplies, *J. Am. W. W. Assoc.*, 1933, **25**, 1495–1504. Fair, G. M., On the determination of odors and tastes in water, *J. New England W. W. Assoc.*, 1933, **47**, 248–272. Fair, G. M., and Wells, W. F., The air-dilution method of odor determination in water analysis, *J. Am. W. W. Assoc.*, 1934, **26**, 1670–1677. Gant, V. A., and Shaw, H. D., Odor concentration in air-conditioned structures, *J. Ind. Eng. Chem.*, 1937, **9**, 16–18.

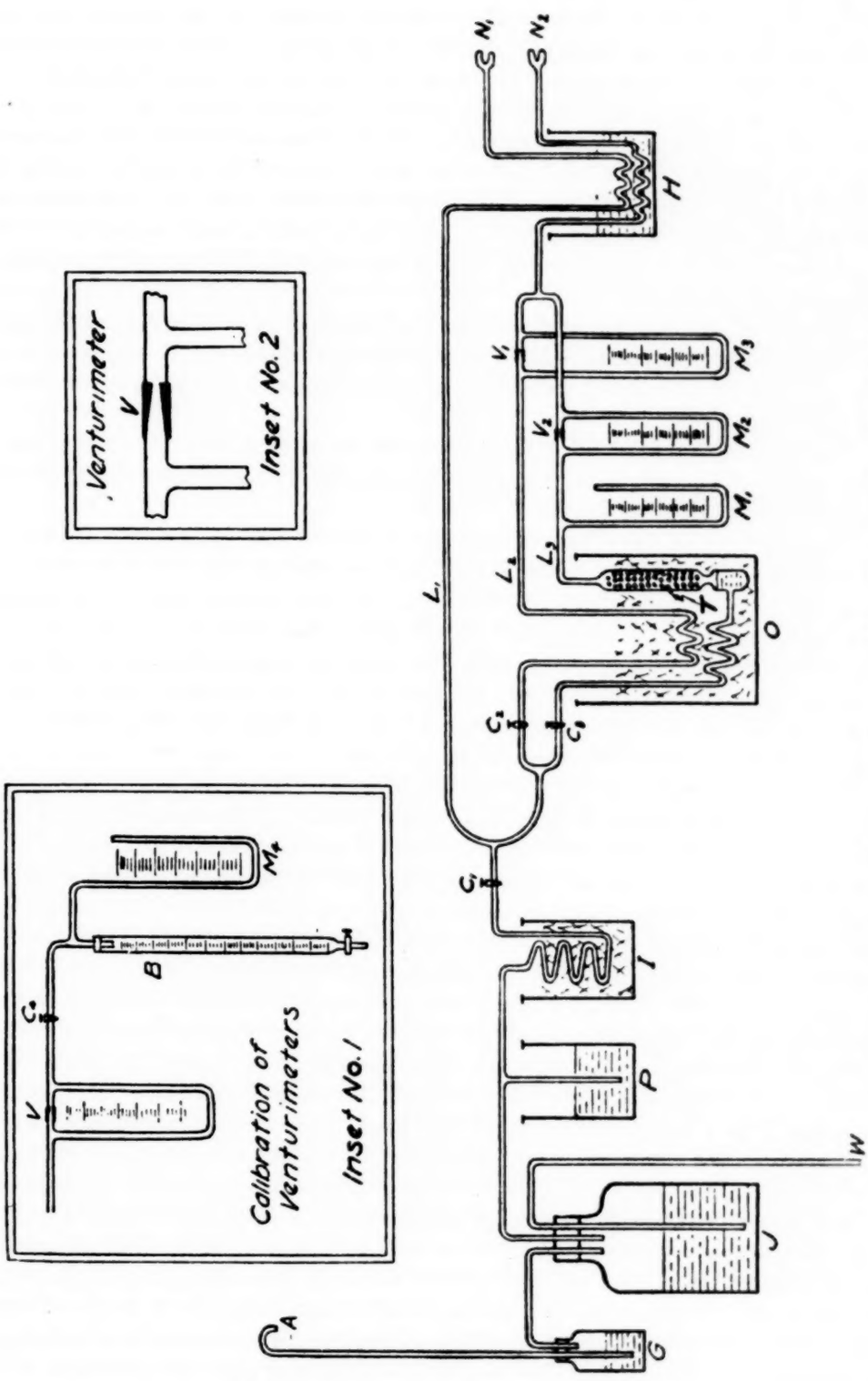

DIAGRAM 1. A device for controlling the flow and concentration of odors.

When the carboy was filled with water, the tube was detached from the faucet and the carboy emptied. As the water level was lowered, air was sucked into the vessel through an air trap which consisted of a small jar (*g*) partially filled with potassium permanganate, the purpose of the latter being to oxidize organic impurities.

This steady stream of pure air was passed through a cooling coil placed in a small tank (*I*) containing ice and rock salt. Most of the water vapor was thus condensed out and the air was dried and at the same time pre-cooled before passing to a dividing point. Here the single line diverged into three. The first L_1) contained only pure air, and, after being warmed in a hot water bath to blood temperature, the air emerged through a nosepiece (N_1) located beyond a small screen. This provided a standard for comparison.

The second line (L_2) was used to dilute the stimulus vapor. Air passing through the third line (L_3) was bubbled through the odorous substance which was placed in a tank (*O*) of salt and ice which could be held at any desired temperature for relatively long periods of time.

The air in both the second and third lines was passed through cooling coils placed in the same freezing tank (*O*) so that the air picking up the odor would be at the same temperature as the test substance.

The absolute pressure in the odor line was measured by an open manometer (M_1). The relative pressures in the odorous line and the dilution line were measured by means of other manometers (M_2 and M_3). The two legs of the manometers extended down on either side of a constriction in the tubing (see Inset 2). Lines two and three were joined and the resulting single line contained a mixture of vapor and pure air whose molar concentration could be determined. As the air in this line was warmed by passing through a tube immersed in the hot water bath (*H*), the subjects were allowed no cue from differences in temperature.

The relative pressure manometers were calibrated as follows. They were isolated and air was passed over one of them at a certain pressure. (See Inset 1 for set-up of calibration.) The flow line was connected with a burette (*B*). The outlet on the burette was opened just enough to permit water to escape at the same rate at which the air entered at the top. Any variations in pressure could be noted by means of a simple water manometer (M_4), which was attached to a T-tube in the line, and quickly corrected. After a bit of practice the experimenter became quite facile in regulating the apparatus so that pressure changes were practically eliminated. This consisted in lowering the beaker holding the outlet water at the rate of fall of the water in the burette, the manometer being constant. The amount of water passing through the burette in one minute was taken as an index of the rate of flow of air. The rate of flow varied from a minimum of 7 cc. per minute in L_3 to 2700 cc. per minute in L_2.

Almost the entire apparatus was made of glass. Rubber tubing had to be used for a few of the joints, but in each case it was thoroughly washed to remove the dressing and then coated thickly with water glass (sodium silicate) which is odorless. In most places glass sleeves instead of rubber sleeves were used to seal joints. The apparatus was taken apart and cleaned with hot water after every fifty trials; it was dried with compressed air. Pure air was allowed to blow through all parts of it after every fifteen trials. To ensure the purity of the air source, the potassium permanganate was changed from time to time and the carboy washed with soap and hot water.

A modification of the method of constant stimuli was used in testing the subjects. The usual method consists in determining the threshold separately for a number of individuals. Each subject makes a large number of observations at different settings of the stimulus pairs whose values are unknown to him. More than one individual is

employed as subject since individuals may vary widely in their threshold. The item of concern however is not ordinarily the threshold of any single individual, but the general, typical value. What is really wanted is a sort of average of the thresholds and their precisions.

Because of the peculiarities of olfactory experimentation, it has not been possible to employ the standard constant method. The modifications have been made in the direction of what the ultimate significant figure should be. The main factor responsible for the changes in method is the difficulty in rapidly modifying the stimulus-intensities. The temperature of the freezing material, controlled by salt and ice, must be fairly constant, and could be set and held fairly constant for from one to two hours. Of course the apparatus allows for adjusting the amount of air in the two lines L_3 which is odorous, and L_2 which is dilutional. But as a matter of fact the flow of gas (air) in the odorous line was usually less than 50 cc. per min. and often as low as 7 cc. per min., while in the dilution line the flow varied from 2000 to 2400 cc. per min. It is rather difficult to adjust the relative flows from moment to moment, and return each time to some constant setting. It was considered appropriate to set the manometers carefully, by use of the flow cocks, and leave the settings for all the judgments at that value. So, instead of having each subject make a number of consecutive observations at different settings of the apparatus, it became necessary to have a large number of subjects make one observation at each fixed setting of the apparatus. The values which are obtained consist in a sort of collective cumulative distribution curve. The limen for no individual is determined, but the approximate median threshold value is established, together not with its precision, but with the distribution of thresholds in the sampled population.

The number of subjects employed at any setting was again determined by the limitations of the apparatus. Since the set of the apparatus for a given temperature could be maintained constant only for about two hours, the work had to be accomplished within this limit. It was necessary to run the subjects with considerable dispatch, and to routinize the process of cleaning the apparatus by blowing it out with clean air. Under these circumstances, about fifty subjects was the limit that could be employed at one time.

Subjects were taken from several classes in psychology. The total population of subjects on any odor was about 150. They were sent one at a time from the classroom to the experimental room, and as many went as could during the hour. Thus, although exactly the same persons did not serve as subjects on each setting for each odor, the same general group was employed. The apparatus was thus set for a certain concentration and fifty subjects were tested. The stimulus intensity was then changed, and fifty more subjects were tested. There was the possibility that, since most of the subjects were naive, as they became familiar with the procedure their ability to give judgments might vary. To cancel out any such practice effects, the subjects who were tested at a low concentration on one occasion were tested at a high concentration on the next occasion.

The subjects were asked to inhale briefly and gently at each of the two nosepieces and to report which if either of them contained an odor.

To the extent that it was possible, the method of inhalation was standardized. The two nosepieces were so constructed as to prevent any but the stimulus vapor from entering the nostril. They were sterilized between trials with a 70 percent solution of alcohol and then carefully dried to make sure that there remained no residue of alcohol to confuse the subject in his judgments. The entire apparatus was set under a large ventilating hood.

As a consequence of the controls employed, we may use the following formula for this determination:

$$\frac{(\text{V.P.})\ (\text{P}\ L_3)}{(\text{T.P.})\ (\text{P}\ L_3)\ (\text{P}\ L_2)},$$

V.P. refers to the vapor pressure of the odorous substance at that temperature.

P L_3 refers to the pressure in the odor line.

T.P. refers to the total pressure in the odor line as measured by the open manometer.

P L_2 refers to the pressure in the dilution line.

The molar ratio or number of mols of odorous material per mol of air is obtained by this formula. The ratios for the four odors investigated are summarized below:

TABLE II

Quinoline			Methyl Salicylate		
Number of Subjects	% of Ss Who Detected Odor	Molar Ratio	Number of Subjects	% of Ss Who Detected Odor	Molar Ratio
16	12	.0000014	50	22	.0000137
42	23	.0000018	50	40	.0000183
15	26	.0000033	42	60	.0000202
50	40	.0000029	40	67	.0000289
50	47	.0000044	50	80	.0000353
40	55	.0000069			
30	60	.0000071			
50	66	.0000077			
31	77	.0000095			

Turpentine			Camphor		
Number of Subjects	% of Ss Who Detected Odor	Molar Ratio	Number of Subjects	% of Ss Who Detected Odor	Molar Ratio
35	14	.000000756	47	10	.000000045
60	40	.000000863	43	30	.000000067
50	56	.000001056	47	57	.000000084
50	88	.000008664	50	80	.000000906

The median values of sensitivity computed by the linear interpolation method, are as follows:

Odorous Substance	Approximate Threshold Terms of Molar Ratio
Quinoline	.0000053
Methyl salicylate	.0000192
Turpentine	.00000098
Camphor	.000000079

Although the curves for turpentine and camphor are asymptotic and are roughly of the same shape, the values for camphor are approximately ten times smaller. The curves for quinoline and methyl salicylate are much more normal ogives, although their precision is quite different.

It appears that the apparatus as constructed can be employed for the determination of olfactory thresholds with a wide variety of substances. When a considerable array of thresholds has been determined, it may be possible to formulate some classification of odors based on the relative strengths necessary for perception. The possibility of a prompt solution of this problem is remote, because the vapor pressures have been determined only for a limited number of substances. A further difficulty is the fact that many workers have used such highly complex substances as cheese, rubber, etc. in making their comparisons. In many cases not only are the substances quite unstable but the chemical formulæ are unknown.

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STUDIES IN CONFIGURAL CONDITIONING. III. THE FACTORS OF SIMILARITY, PROXIMITY, AND CONTINUITY IN CONFIGURAL CONDITIONING

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The Problem

In a previous study by the writer (4) it was found that salivary configural conditioning—conditioning to a compound stimulus with no conditioning to the component stimuli of the compound—with lights as the stimuli to be conditioned manifested itself much more when the lights were of different colors than when they were of the same color. This finding was obviously at variance with the principle of Similarity in perception—stressed, if not originally enunciated, by the Gestalt psychologists—in accordance with which similar stimuli (or fields) possess closer grouping unity. Of course, the Gestalt principles of grouping were derived in a domain quite different from that of conditioning. Still and even because of this, it was thought highly significant to investigate to what extent, if at all, these principles would operate in the patternization of a glandular response which is not only objectively and quantitatively measurable, but which *as such* is presumably not influenced in its selective grouping by past experience. At any rate, it was desired to discover what relations between the components of a compound stimulus favor configural conditioning, and Similarity, Proximity, and Continuity were good starting hypotheses.

Procedure

The writer's general salivary CR technique with adult human subjects has been described before (3–4) and will not be further detailed. It consists, first, of measuring salivation by increments in weights of dental cotton rolls (Johnson and Johnson, No. 3) 1.5 × 0.5 in. inserted under the S's tongues for a short period of time, usually one minute. To prevent evaporation, scale-corrosion and absorption, the rolls are

weighed in small envelopes and reweighed in the envelopes immediately after the removal of the cotton from the Ss mouths. Since the cotton-in-the-mouth is by no means a totally inactive stimulus, periods of control salivations must always be rotated with experimental periods, but the weighing itself need not be more accurate than to one centigram, mg. accuracies being as a rule a superfluous refinement. Secondly, the technique involves presenting a *series* of stimuli during eating periods of 2–4 minutes and misinforming the Ss about the purpose of the experiment so as to forestall disturbing subjective attitudes and association-sets. Series of stimuli and comparatively long eating periods are used, as misinforming is unsuccessful and association-sets can hardly be averted with the traditional single-conditioned-stimuli-plus-brief-reinforcement procedure. 'Effects of eye-fatigue upon digestion,' 'influence of music upon salivation,' and diverse imaginary 'learning and memory' problems have been some of the simulated 'purposes of the experiment' in the past.

The special feature of the procedure of the present experiment was of course the varying of the components within the to-be-conditioned compound stimuli. 2-stimuli compounds and 3 variations of these stimuli with each S were most feasible with the set-up, and 4 sets of variations were in all used with the 8 Ss of the experiment, 2 Ss for each set. Specifically, with Ss A and B the 3 variations of the components in the compound stimulus were: (*a*) flashings of 2 small green electric bulbs, 2 c.p. and 0.5 inches in diameter, placed at a distance of one inch from each other; (*b*) flashing a red and a green bulb of similar size and power and at a similar distance from each other; and (*c*) flashing a green bulb of 2 c.p. and 0.5 inches in diameter with a red lamp of 16 c.p. and one inch in diameter. The grouping factor studied here was Similarity, the component stimuli being similar in all respects in (*a*), differing in color in (*b*) and differing in color, size, and intensity in (*c*). With Ss C and D Proximity was investigated, the red and green bulbs having been placed at distances of one inch, 2 inches, and 4 inches from each other. Again, the 3 variations in Ss E and F were: (*a*) a red and a green bulb as in the second variation of the first 2 subjects; (*b*) a similar green bulb combined with the sound of a small doorbell; and (*c*) a green bulb combined with the exposure of nonsense syllables 'Zag Dih.' Finally, in Ss G and H the factor of Continuity was experimented with. Two larger bulbs, one red and one green, were placed at a distance of 3 inches from each other, and one or two miniature flashlight bulbs (2.5 volts, 7/16 inches in diam.) were interposed between them in 2 of the 3 variations. All bulbs were globular in shape, and were screwed through a cardboard exposure screen so that their bases were out of Ss sight.

In all Ss the eating periods lasted for 3 minutes, chewing gum, sucking at lollipops, and eating pretzels having been the usual food. During these periods the respective compound stimuli were applied 45 times, each application lasting for one second and separated from its next by 1–5 seconds. The 3 variations of the compound stimuli were rotated, the 45 applications permitting nearly 3 perfect permutations of them. After the eating periods came testing periods of 7–9 minutes. During these periods the Ss salivations in one-minute periods were ascertained for the action of each of the 3 variations of the compound stimuli, the several component stimuli, as well as for two mere cotton-in-the-mouth control periods. Of the 2 control periods, one always followed the eating and merely served to eliminate the excess of accumulated saliva, while the other, the 'regular' control period was temporally rotated with the experimental periods and used as a subtrahend in determining the net amount of conditioned salivation. Again, in the experimental one-minute testing periods the number of conditioned stimuli—whether compound or components—applied was

always 15, one-third of that given in the 3-minute training or eating periods. Finally, a testing period of 7–9 minutes and an eating-and-to-be-conditioned-stimuli-observing training period of 3 minutes constituted one trial, and in all 5 trials were made in each experimental session and 6 sessions or 30 trials with each S. The Ss were college undergraduate, paid by N. Y. A. 50 cents per hour, and the experiment was conducted in a dimly lighted, fairly sound-proof room, from 3 to 5 o'clock in the afternoon. The distance between the Ss and the exposure screen was about 2 feet, and the screen was divided into 3 parts for the 3 variations of the compound and moved for each variation so as to eliminate possible space differentials.

Results and Discussion

The results are presented in Tables 1–5. In Tables 1–4 are given, trial by trial, the conditioned salivations in centi-

TABLE 1

Compound and Componental Salivary CRs of Two Adult Human Ss as Affected by the Similarity between Component Stimuli

Figures represent salivations in centigrams per one-minute periods (commas set off the salivations of each S).

No. of Trial	Compound Stimuli			Component Stimuli		
	2 Green Lights (2 c.p. each)	A Green and a Red Light (2 c.p. each)	A Green and a Red Light (2 and 16 c.p.)	A Green Light (2 c.p.)	A Red Light (2 c.p.)	A Red Light (16 c.p.)
1	3, 5	8, 6	4, 7	0, 0	1, 3	4, 6
2	10, 13	14, 18	11, 15	−3, −1	0, 0	2, 0
3	3, 8	10, 17	13, 18	2, 1	3, 6	2, 5
4	5, 8	19, 23	10, 11	4, 7	2, 7	3, 9
5	4, 9	11, 26	15, 19	3, 8	4, 1	2, 0
6	8, 9	15, 18	14, 19	2, 8	1, 8	3, 10
7	10, 11	23, 25	12, 9	6, 8	2, 9	4, 8
8	5, 10	22, 18	11, 17	4, 8	3, 7	8, 9
9	7, 8	34, 25	10, 4	9, 2	4, 1	2, 0
10	10, 12	17, 24	9, 6	14, 6	8, 9	0, 0
11	14, 17	18, 31	8, 9	5, 8	0, 0	7, 9
12	8, 5	16, 19	9, 0	4, 6	−1, 5	8, 6
13	4, 9	21, 20	18, 24	8, 9	3, 0	7, 9
14	5, 8	20, 20	14, 17	10, 11	4, 8	3, 2
15	8, 7	16, 12	18, 19	7, 6	0, 5	9, 10
16	5, 8	11, 13	23, 24	9, 10	7, 9	11, 14
17	9, 10	16, 18	14, 12	8, 7	6, 6	12, 15
18	8, 1	10, 18	11, 15	12, 14	8, 6	15, 16
19	7, 7	14, 19	18, 16	13, 10	3, 9	15, 17
20	10, 13	26, 21	28, 19	3, 12	8, 9	11, 6
21	8, 4	20, 8	17, 8	4, 18	14, 11	18, 8
22	5, 8	19, 8	17, 18	11, 15	18, 14	15, 12
23	14, 12	11, 7	13, 11	8, 4	4, 1	11, 13
24	12, 10	14, 17	12, 19	7, 0	3, 4	10, 13
25	9, 2	10, 11	10, 15	24, 16	18, 17	14, 11
26	8, 10	14, 18	13, 18	20, 9	14, 10	19, 23
27	7, 9	10, 9	14, 19	11, 10	13, 23	24, 10
28	5, 4	19, 21	23, 24	10, 11	18, 19	20, 10
29	8, 9	11, 20	25, 21	10, 16	18, 10	20, 23
30	0, 3	3, 8	17, 10	18, 14	11, 16	23, 14

grams per one-minute periods for each S and each variation of compound and component stimuli. The conditioned salivations are of course always the differences between salivations in control and conditioned periods, with the latter as the

TABLE 2

COMPOUND AND COMPONENTAL SALIVARY CRs OF TWO ADULT HUMAN Ss AS AFFECTED BY PROXIMITY BETWEEN COMPONENT STIMULI

Figures represent salivations in centigrams per one-minute periods (commas set off the salivations of each S). A green and a red light (2 c.p. each) were the stimuli throughout the experiment.

No. of Trial	Compound Stimuli			Component Stimuli	
	Lights at Distances of One In.	Lights at Distances of 2 In.	Lights at Distances of 4 In.	Green Light	Red Light
1	21, 16	15, 19	8, 4	0, 6	−4, −3
2	3, 8	11, 9	4, 2	0, −3	−3, −5
3	5, 9	10, 18	3, 8	7, 9	6, 10
4	10, 14	3, 14	2, 9	4, 10	2, 14
5	14, 8	12, 8	4, 2	2, 18	3, 24
6	19, 2	26, 5	5, 0	5, 15	3, 23
7	32, 5	21, 4	9, 0	4, 18	−3, 10
8	34, 19	24, 6	8, 2	3, 18	2, 28
9	17, 12	23, 17	7, 6	4, 18	6, 15
10	14, 11	24, 16	8, 12	8, 17	7, 11
11	15, 12	28, 17	0, 0	2, 8	0, 14
12	3, 19	8, 10	3, 6	1, 16	4, 9
13	4, 0	8, 10	2, 8	4, 7	3, 18
14	2, 8	19, 13	4, 0	3, 13	8, 16
15	11, 2	18, 1	11, 6	4, 18	7, 19
16	15, 14	13, 19	16, 10	12, 15	8, 14
17	10, 17	12, 16	13, 13	11, 10	9, 6
18	23, 14	28, 21	18, 14	12, 9	3, 7
19	12, 19	26, 31	17, 14	18, 14	2, 23
20	11, 23	27, 34	11, 10	22, 14	21, 18
21	14, 18	20, 24	9, 2	14, 13	20, 18
22	17, 23	29, 30	16, 11	17, 12	9, 15
23	16, 12	22, 35	14, 11	8, 3	5, 14
24	11, 15	26, 28	18, 19	9, 10	11, 16
25	14, 13	32, 19	19, 9	3, 2	0, 10
26	8, 4	11, 10	9, 7	12, 15	18, 13
27	5, 6	10, 18	4, 1	3, 8	8, 6
28	4, 1	2, 1	10, 6	21, 16	14, 13
29	2, 7	11, 16	10, 14	22, 34	14, 11
30	4, 19	23, 28	15, 24	14, 9	20, 6

minuends. In Table 5 are offered the mean conditioned salivations of all trials and Ss for each compound stimulus, mid-component stimuli (means of the 2 component stimuli), and the ratios of compound to mid-component conditioning. While these means pose some summary view of the findings,

they do not, however—because of the highly dynamic nature of the forms of compound conditioning—fully illumine their nature and developmental course which could only be gained

TABLE 3

COMPOUND AND COMPONENTAL SALIVARY CRs OF TWO ADULT HUMAN Ss AS AFFECTED BY CONTINUITY BETWEEN COMPONENT STIMULI

Red and green lights of 16 c.p. and one inch in diameter were the main stimuli; white miniature flashlight bulbs (2.5 volts, 7/16 in. in diam.) were the continuity stimuli. Figures represent salivations in centigrams per one-minute periods (commas set off the salivations of each S).

No. of Trial	Compound Stimuli			Component Stimuli			
	No Continuity Stimuli	One Continuity Stimulus	Two Continuity Stimuli	Green Light	Red Light	One Continuity Stimulus	Two Continuity Stimuli
1	0, 1	2, 7	4, 8	0, −6	4, 2	1, 1	0, 0
2	2, 4	1, 0	3, 2	0, 0	0, 0	3, 0	−4, 2
3	11, 15	14, 15	12, 14	5, 6	2, 0	−2, −5	−3, 1
4	18, 19	23, 24	17, 29	6, 1	3, 4	1, 0	−1, 2
5	23, 17	28, 14	19, 34	8, 6	5, 5	2, 1	3, 1
6	24, 10	34, 32	18, 6	9, 0	2, 3	−4, −2	1, 3
7	16, 4	30, 8	19, 36	4, 0	3, 1	−3, −5	2, 0
8	17, 5	31, 19	32, 25	8, 1	10, 3	−6, −3	1, −7
10	18, 6	32, 14	33, 11	9, 4	11, 5	−1, 1	3, −3
11	10, 9	26, 20	25, 34	8, 7	12, 9	3, −8	−2, −4
12	21, 15	23, 18	29, 34	9, 6	11, 4	−5, −4	3, −7
13	11, 17	25, 19	32, 33	17, 18	9, 6	3, 7	−5, −3
14	16, 8	24, 23	18, 35	26, 16	3, 8	−9, −5	−8, −1
15	18, 5	23, 30	18, 33	22, 14	4, 8	−4, −2	1, 0
16	14, 18	22, 30	16, 18	10, 4	17, 11	−4, −8	2, 0
17	13, 17	24, 32	26, 11	16, 19	11, 10	8, −7	3, 2
18	14, 15	23, 22	32, 36	18, 10	10, 6	3, 2	1, 1
19	19, 10	24, 25	24, 26	17, 15	11, 18	6, 3	2, −4
20	23, 24	34, 30	29, 28	16, 21	10, 9	7, 1	−4, −4
21	22, 18	30, 34	32, 35	10, 14	11, 16	8, −9	−7, −3
22	23, 19	32, 17	34, 35	19, 16	3, −4	2, −6	−8, −6
23	11, 18	31, 12	18, 35	23, 32	19, 15	−9, −7	−4, −8
24	18, 16	30, 14	16, 19	20, 13	14, 15	3, 9	−8, −7
25	11, 10	34, 30	18, 32	22, 19	9, 4	6, 9	2, 0
26	17, 9	30, 34	23, 31	24, 10	10, 3	−8, 3	−4, −7
27	14, 17	14, 19	18, 32	29, 11	10, 13	−4, −2	−6, −8*
28	18	23	19	7	8	3	2
29	4	18	23	18	2	−8	4
30	11	16	22	7	3	10	11

* Subject F missed the last trials

by trial-by-trial examinations of individual Tables 1–4. Similar distortions enter the ratios of mean compound to mean mid-component conditioning, since as a rule in the course of training the CRs to compound stimuli decrease and those

to the component stimuli increase. Still, the main differences between the configural attributes of the various compound stimuli are not altogether obscured in Table 5, which is the most instructive single table of the group.

TABLE 4

COMPOUND AND COMPONENTAL SALIVARY CRs OF TWO ADULT HUMAN Ss AS AFFECTED BY SIMILARITY BETWEEN COMPONENT STIMULI

Figures represent salivations in centigrams per one-minute periods (commas set off salivations of each S).

No. of Trials	Compound Stimuli			Component Stimuli			
	Green and Red Light (2 c.p. each)	Green Light and Bell	Green Light and Nonsense Syllable	Green Light	Red Light	Bell	Nonsense Syllable
1	3, 3	0, −4	2, 8	0, 9	1, 5	2, 0	3, 5
2	10, 14	1, −2	1, 11	2, 7	10, 4	3, 4	7, 8
3	16, 11	3, 1	20, 13	4, 6	11, 2	4, 0	3, 4
4	18, 10	2, 4	23, 15	1, 2	5, 3	5, 8	6, 1
5	11, 23	3, 12	25, 19	3, 4	8, 1	4, 1	0, 0
6	18, 16	2, 17	22, 18	4, 5	6, 4	2, 0	0, 0
7	13, 11	6, 10	16, 12	0, 0	3, 1	3, 6	−8, −4
8	3, 6	8, 4	12, 28	13, 5	4, 3	4, 7	2, 5
9	3, 5	2, 8	14, 23	11, 10	3, 4	7, 8	3, 7
10	8, 1	4, 1	17, 8	14, 18	4, 5	8, 2	6, 8
11	12, 5	4, 8	14, 11	10, 5	8, 6	7, 4	10, 11
12	16, 23	7, 9	19, 13	14, 18	12, 19	18, 3	14, 19
13	22, 15	9, 8	13, 14	11, 16	13, 10	4, 1	12, 16
14	14, 19	22, 14	18, 11	10, 15	15, 18	6, 7	14, 18
15	19, 15	4, 10	13, 19	11, 18	12, 17	8, 4	13, 24
16	28, 10	11, −4	14, 38	17, 14	14, 23	7, 7	14, 28
17	20, 17	13, −5	18, 19	23, 18	17, 24	8, 13	17, 19
18	14, 10	6, 4	24, 17	22, 18	19, 16	5, 16	15, 23
19	21, 17	8, 3	23, 14	10, 4	17, 10	4, 8	11, 20
20	16, 13	8, 9	20, 14	13, 8	19, 10	3, 18	23, 28
21	18, 14	3, 8	26, 9	16, 7	12, 16	6, 15	22, 19
22	23, 10	4, 9	23, 18	15, 11	9, 8	4, −1	15, 12
23	9, 18	3, 2	24, 19	19, 24	17, 13	3, 2	16, 14
24	8, 7	5, 6	23, 15	8, 8	16, 11	16, 4	20, 13
25	4, 17	4, 4	20, 16	14, 16	23, 19	7, 8	23, 17
26	11, 15	7, −5	23, 18	18, 11	24, 18	23, 11	24, 29
27	18, 0	8, 3	16, 11	23, 14	25, 17	3, 9	23, 9
28	14, 7	3, 4	15, 19	25, 18	18, 0	3, 0	21, 6
29	11, 16	14, 11	23, 10	8, 11	18, 8	4, 2	24, 8
30	14, 19	17, 12	24, 25	32, 8	7, 4	3, 4	16, 9

As seen from the results, some answer may be made out now to the questions set forth at the beginning of this paper. In the main, it appears that, at least in the form fostered by the Gestalt psychologists, the factors of Similarity, Proximity,

and Continuity do not hold for configural conditioning. True, there was more configural conditioning—as measured, for instance, by the ratio of compound to component conditioning—when the stimuli in the compound were of the same modality, red and green lights, than when they were of different modalities, green light and bell, and also more when the stimuli differed only in color than when they differed in color, size, and intensity. But, on the other hand, a red and a

TABLE 5

MEANS AND RATIOS OF MEANS OF THE CONDITIONED SALIVATIONS OF 8 ADULT HUMAN Ss TO VARIOUS COMPOUND STIMULI AND THEIR COMPONENTS

Each figure is the means of 2 Ss and 60 trials (in centigrams per one-minute periods).

Ss	Nature of Compound Stimulus	Conditioned Salivation to Compound Stimulus	Mid-component Conditioned Salivation	Ratio of Compound to Mid-component Conditioned Salivation
A and B	2 Green Lights (2 c.p. each)	7.8	8.3	0.94
" " "	A Green and a Red Light (2 c.p. each)	16.4	7.8	2.10
" " "	A Green and a Red Light (green 2 c.p.; red 16 c.p.)	14.6	9.1	1.60
C and D	Green and Red Lights (2 c.p.) at One in. dist.	11.8	10.2	1.15
" " "	Green and Red Lights at 2 in. Distance	17.4	10.2	1.76
" " "	Green and Red Lights at 4 in. Distance	8.5	10.2	0.83
E and F	Green and Red Lights (36 c.p.) with No Continuity Lights	13.7	9.4	1.45
" " "	One Continuity Light	22.6	8.0	2.82
" " "	Two Continuity Lights	23.7	8.4	2.80
G and H	Green and Red Light (2 c.p.)	13.1	11.4	1.15
" " "	Green Light and Bell	5.1	8.2	0.62
" " "	Green Light and Nonsense Syllable	17.2	11.9	1.44

green light were configurally stronger than two green lights and a green light plus a nonsense syllables than a green and a red light (Tables 1, 4, and 5). Similarly, compound conditioning was more effective with a distance of 2 inches between the light stimuli than with either one or 4 inches, while the differences between interposing one and two continuity lights were very slight (Tables 2, 3, and 5). The relationship between these factors and configural conditioning is obviously

not uni-directional but bi-directional in kind. Apparently, to produce best compound *per se*—configural—conditioning, the stimuli in the compound must not be very similar, near, and continuous to each other, but, on the contrary, in some way dissimilar, opposing, removed, and discontinuous. Then, the optimum degree of similarity-dissimilarity seemingly varies with the nature of the total task, as manifested by the differences between Ss A, B and G, H.

Juxtaposing these results with the writer's previous findings (**4**–**5**) and with the considerable number of experiments on compound conditioning in animals reviewed elsewhere (**4**), a few generalized statements are suggested. First, that supra-summative pattern-action is a function of certain specific relations between the separate parts of the pattern. Second, that while these relations are highly dynamic and functional, they still are experimentally determinable and measurable. Third, that both parts and patterns may be genuine determinants of behavior; that either one affects the other and either one may emerge from the other, that individuation and integration are complimentary stages of development. These statements, it may be seen, are quite out of step with the tenets of the Gestalt theorists, and the writer confesses that he found their voluminous writings of little concrete aid in accounting for the phenomena he has studied and observed.[1]

Summary

1. Salivary CRs have been established in 8 adult human subjects to compounds of stimuli and to the components stimuli of the compounds.

2. All compounds consisted of two stimuli and of three variations in similarity, proximity, or continuity. With Ss A and B the conditioned stimuli were: two green lights, a red and a green light of equal size and intensity, and a red and a green light of differing size and intensity. With Ss G and H

[1] The writer sees little import in the contention that the phenomena which he has studied are arbitrary or artificial and thus not within the suzerainty of Gestalt principles. In what way is a S's learning to secrete saliva at the flash of a light or a dog's receiving food at the sound of a melody significantly more artificial than some of Man's most important life-adjustments, let us say, the acquisition of a language.

they were: a red and a green light, a green light and a bell, and a green light and a nonsense syllable. With Ss C and D the red and green lights were placed at distances of one, two, and four inches, while with Ss E and F one or two small continuity lights were interposed between the main stimuli lights.

3. The order of effectiveness of the compound conditioning within each of the three variations of the stimuli was: a) red and green lights equal in size and intensity, red and green lights unequal in size and intensity, green lights alone; b) green lights and nonsense syllables, green and red lights, green lights and bells; c) green and red lights at distances of two inches, one inch, four inches from each other; d) one continuity light between main stimuli, two continuity lights, no continuity lights between stimuli.

4. It is concluded that supra-summative compound or configural conditioning is best produced when the component stimuli are in some way dissimilar, removed, and discontinuous and that the Gestalt principles of Similarity, Proximity, and Continuity do not hold in a situation in which past experience and present selective motivation is ruled out.

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A NEW ISOLATION CABINET FOR INFANT RESEARCH

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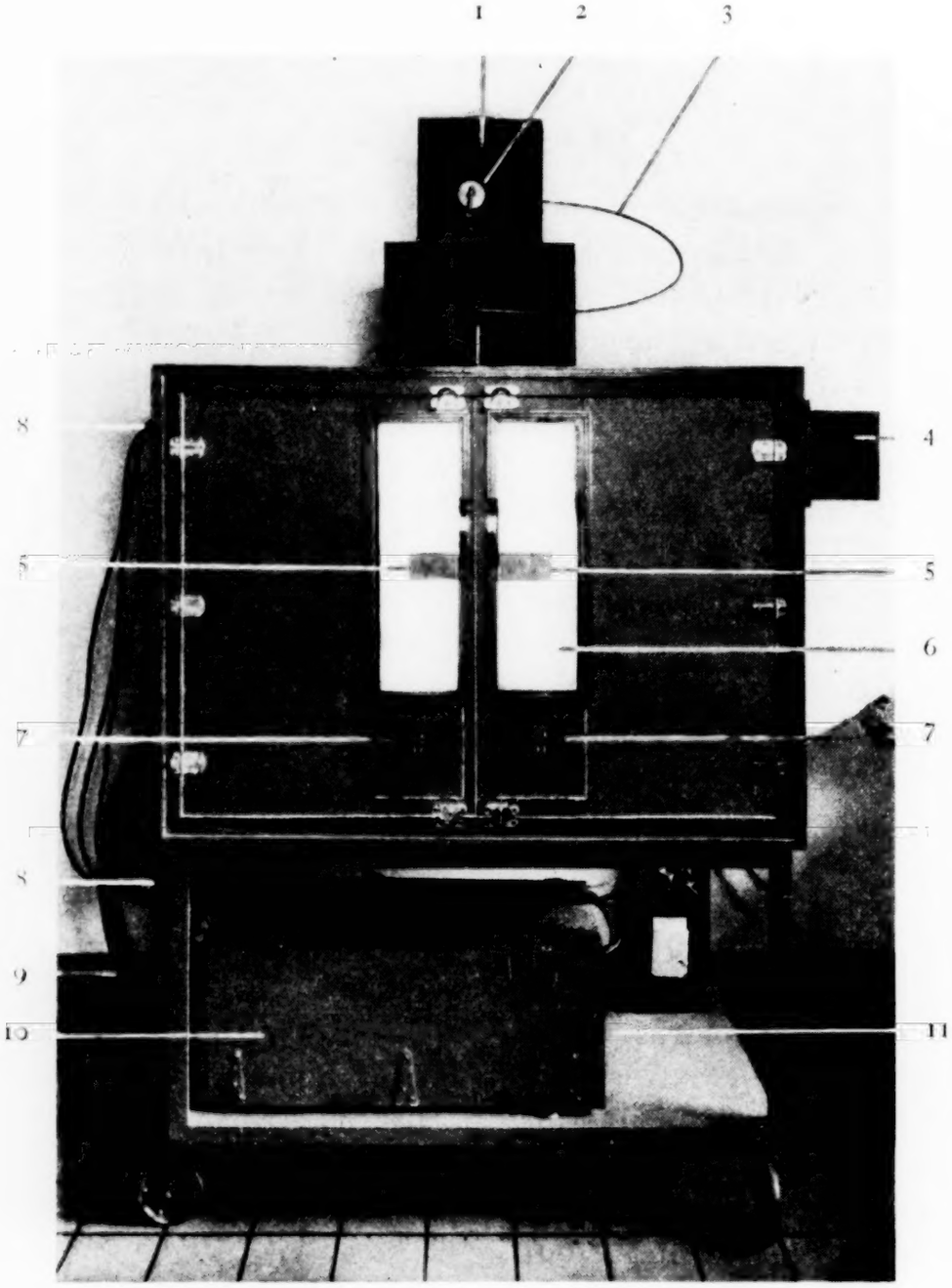
The isolation cabinet devised by Dr. Weiss and his students has been in constant use since their first investigations.[1] Necessarily it has undergone modifications from time to time until it seemed advisable to rebuild the entire cabinet. It is important in a cabinet of this sort that we have control of temperature, humidity, light and sound. It is not absolutely essential to secure perfect silence, but it is necessary to eliminate disturbing noises and those sounds that might be picked up by any recording apparatus. Shielding against stray electrical disturbances is also important in case investigations of electrical phenomena are being pursued. Relatively constant control of temperature and humidity are of great importance as the infant undressed is disturbed by the rapid evaporation on the skin.

The new cabinet was designed to meet these requirements.[2] The inside dimensions are 30″ × 40″ on the floor and 30″ high. The whole cabinet is mounted on a table constructed of a framework of 1½-inch angle iron welded together. It is of sufficient height so that when mounted on 5″ casters the floor of the cabinet is 30″ high. There is a shelf at the base of the table for batteries, transformer, air conditioning unit and other auxiliary equipment. The frame of the cabinet is constructed of 2″ × 2″ pine. The inside walls are of ½″ masonite soft board to which are attached irregular blocks to dampen vibration. The outside walls are of masonite pressed board and the intervening space is firmly packed with rock

[1] Pratt, K. C., Nelson, A. K. and Sun, Hugo H. The behavior of the newborn infant. *Ohio State University Studies*, Contributions in Psychology No. 10, the Ohio State University Press, 1930.

[2] We are indebted to Dr. Robert Walker and several graduate students for numerous helpful suggestions during the construction of this cabinet.

wool to deaden further the sound and ensure heat insulation. The inner walls, including doors and other openings, are completely covered with a 16-mesh copper screen which is



grounded. Over this, with the exception of observation ports, is tacked white quilted mattress padding which can be easily removed for laundering. There are two air intakes near the bottom at one end with a baffle of soft board 6″ high

across the cabinet to prevent direct draft upon the infant and to reduce the transmission of sound. At the top of the opposite end are corresponding air outlets which connect with a chamber (4) built onto the outside and adequately baffled to muffle sound.

Two doors completely fill one side. These can be closed and tightly locked by means of window fasteners at the top and bottom of each door. In the lower part of each door is a small friction-type door (7) 6″ square which can be opened for purposes of stimulation or adjustment of the infant and also can be used for connecting mechanical recorders directly to the polygraph or other equipment which is mounted on a stand of the proper height. Above these small doors, extending the remainder of the height of the large doors, are observation ports (6) covered by glass both on the outside and inside of the cabinet. A screen with a small slit which can be adjusted at a convenient height for observation is attached over each of these ports (not shown in the illustration). In the back of the cabinet are two ports (5) 2″ × 5″ for other observers. For some experiments it is more important to have close contact with the infant than to have complete isolation. For this purpose a dark curtain is attached to the top of the front of the cabinet and suspended over the open doors and down to the floor. This makes a small chamber in which the observer can sit and be fairly well isolated with the infant.

Directly in the center of the top of the cabinet is a port 12″ × 12″ with two plates of glass as in the other ports. The camera is mounted above this port. In order to prevent glare on the glass a box 10″ high insulated with soft board and mounted on soft rubber is installed. A 4″ × 4″ hole in the center of this box is connected to the bottom of the camera housing by a bellows (not shown in the illustration). The camera housing (1) is mounted upon a vertical rod to permit adjustment. This housing is also insulated and a cradle holds the motion picture camera firmly in position. An antenous release (3) for remote control of the camera leads from directly above the doors of the cabinet to the camera housing.

At the left end of the cabinet is a panel (8) for electrical connections with a corresponding panel on the inside at the same position. There is also a tube of ¼″ inside diameter through these panels for connecting any air recording apparatus, such as the pneumograph. On the iron stand immediately below this panel is another (8) to which the main current from a wall plug and batteries is supplied. Here there are also switches for controlling the cold lights.[3] Extension cords connect these two panels according to the need of the investigation.

The air conditioning unit (10) consists of a box 12″ × 12″ × 30″ lined with asbestos. At one end is an opening 4″ in diameter to admit the air (11). Immediately in front of this hole is an exhaust fan which drives the air against a heating unit and then against staggered baffles of soft board and thence through two outlets large enough to admit the ends of the hose of an ordinary house vacuum clearner. A hose from each opening (9) leads to the corresponding openings in the base of the cabinet. In order to increase the humidity the small amount necessary in the hospital room a pan of water is placed under the baffles and ordinary blotting paper tacked to the baffles with one end submerged in the water. The degree of humidity is determined by the number of baffles carrying blotters. A toluene thermostat within the cabinet connected with a relay within the air conditioning unit cuts the heat unit in or out of the circuit, but the fan runs continuously to ensure proper ventilation. A test run for forty-eight hours at 82° F. with a recording thermometer indicated that the temperature did not vary over one degree.

[3] IRWIN, O. C. A cold light for photographing infant reactions with the high-speed motion picture camera. *Child Develop.*, 1931, 2, 153–55.

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DISCUSSION

CONDITIONING AND ATTITUDES

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In the February '38 issue of this JOURNAL appeared a paper by D. D. Wickens entitled 'The transference of conditioned excitation and conditioned inhibition from one muscle group to the antagonistic muscle group.' In the March issue of the same journal was published an article by A. A. Campbell under the name of 'The interrelations of two measures of conditioning in man.' Both these papers criticize the attitudinal or central theory of human conditioning which I have advocated in recent years. While, however, Wickens' criticisms are general and of a nature that hardly misrepresents their merits to the reader, Campbell's discussion not only reveals a general misconception of my theory but also contains specific misstatements of my findings and a very gross distortion of the present status of conditioning in man. An answer and above all a clarification seem to be needed, and I shall take up Dr. Campbell's paper first.

To begin with, Dr. Campbell does not know that the type of his experiment and his specific findings have been preceded by a large number of others. He states: "So far as the writer is aware, there has been no attempt other than the present one to correlate conditioning scores obtained from the conditioning of different reflexes" (p. 241), and "No comparisons have been drawn between the scores of the same subjects on different measures of conditioning" (p. 226). I can think offhand of a score of such experiments and correlations, both when the different reflexes or responses were elicited simultaneously by the same stimuli, and when they were evoked by different stimuli and at different times; also when three or four reflexes, rather than two as in Campbell's study, were used. Indeed, it is the usual procedure in Krasnogorsky's laboratory to experiment with salivary and with motor reflexes on the same children (**25, 26**). In Ivanov-Smolensky's laboratory, shock, food, and exploratory conditionings have been commonly compared (**17, 20**), while occasionally even in Pavlov s laboratory food and shock CRs have

been tried on the same dogs (**16**). I reviewed three of these experiments in some detail (**35**, p. 40–41, 52–56) and abstracted a few others (**11**, **22**, **23**, **24**, **33**; **34**, p. 218). To quote from experiments reviewed by me in English. Juschtschenko's results show a correlation of only 0.105 ± 0.073 between the magnitudes of conditioned salivary and motor reflexes (**35**, table facing p. 40 and second item on p. 97). Korotkin's findings indicate a small positive correlation between trials required to form shock and food CRs (**35**, p. 55), and only 3 of Khozak's 14 subjects manifest the same course of conditioning in shock, food, and exploratory responses (**22**).[1] Ten years later, unaware of all this, Dr. Campbell found, similarly, negligible correlations between the ease of conditioning the knee-jerk and the eyelid responses. Still, he feels sufficiently versed in conditioning to declare a number of my findings 'distinctly atypical,' 'extraordinary,' and my statement about a certain aspect of conditioning as 'obviously without justification.' But to take up Dr. Campbell's contentions specifically.

1. The specific results of my experiment which Campbell considers 'distinctly atypical' are those that show no correlations between the magnitudes of conditioned and unconditioned reflex salivation. As a basis for this consideration, Campbell cites Pavlov who consistently states positive relationships between ease of conditioning and reflex sensitivity. I, too, am familiar with Pavlov's work. But Pavlov worked with dogs and I worked with human beings. *With human subjects, the correlations between these two variables in salivation have typically been negligible or even negative.* To quote again from English reviews. Wolowick's results give a correlation of 0.157 ± 0.111 between conditioned salivation and subsequent reflex salivation to food (**35**, p. 41–42). Chuchmarev's findings—only a small part of which has been summarized in English—indicate a negative correlation between the two magnitudes (**35**, p. 44), and Lenz's extensive experimentation shows the same general tendency (**27**). Neither have the correlations been concordant or even always positive in human subjects with responses other than salivation. Darrow did obtain a high correlation with the GSR (**9**), but Schlosberg (**43**) has only a zero correlation to offer. The numerous investigations on withdrawal from shock have yielded small positive or negligible correlations (**6**, **30**, **45–47**, **48**), and in Campbell's own experiment the correlations between

[1] I shall avoid as much as possible any quotations from Russian literature that has not been abstracted or reviewed.

ease of conditioning and reflex sensitivity are only 0.26–0.29 for the knee-jerk and 0.41–0.47 for the eyelid response. My zero correlations are thus hardly 'distinctly atypical' and are little at variance with past results. Furthermore, these past correlations not only do not constitute evidence against the attitudinal theory of human conditioning but actually lend some support to it (other factors the significance of which I have never denied must not be neglected). We have seen that in salivation the correlations between reflex sensitivity and conditioning were higher for dogs than for human beings. This is also true for shock conditioning, in which case the relationships are also higher for younger than for older children (**30**). Again, with adult human subjects the highest correlations were found in the GSR where attitudinal and symbolic factors are least likely to enter. Finally, in my latest experiments in which the subjects are unaware of attempts to condition them, the correlations between reflex sensitivity and ease of conditioning are quite high (**38–40**).

2. Campbell writes: "Razran identified three types of subjects, positive, negative, and indifferent. These studies [mine and two more which he cites] suggest that individuals, human or animal, fall into distinct groups when compared on the basis of their relative reactivity to a conditioning influence" (p. 225). But what I found was not three types of *subjects* with permanent conditioning characteristics but three types of shifting and combining *responses*. Far from falling into 'distinct groups,' my subjects changed the types of their responses with each conditioning situation, 'with the nature of the stimuli, kinds of instructions, and presence of other subjects in the experimental room' (**37**, p. 104). Campbell is confused when he thinks that my results suggest that conditioning is a trait and that they contradict the view that "the ease of conditioning is not general but relatively specific to each conditioning situation." On the contrary, my findings show even greater conditioning specificity than his own, since my subjects differed in their course of conditioning not only with differing responses and stimuli to be conditioned, but also with the same responses but different stimuli.

I should like to state, however, that I do not mean to say that the ease of conditioning may not or is not, under certain circumstances, *built up* into some sort of general trait, a conditioning sensitivity or a conditioning resistance towards some object or group of objects, some response or group of responses—red handkerchiefs, acquiline noses, abstract paintings; pressing some-

thing, withdrawing from something. My unpublished results (**39–40**) and a critical evaluation of the experiments from Ivanov-Smolensky's laboratory (**17–20**) lead me *now* to suspect that such may be the case. But this view has never been expressed by me before, nor does it follow from my published results. Besides, *building-up* is not opposed by Campbell's and my findings but supersedes them.

3. Campbell's confusion of response types with subject types is even excelled by his confusion of frequency types of individual responses with frequency types of individual subjects. He apparently believes that in my own experiments I based my view of the prevalence of bi-modality in human conditioning upon the frequency type of the distribution of individual differences between my subjects. He says: "Distributions showing distinct bi-modality or multi-modality are exceptional [that this statement is itself in error will be shown later.] and Razran's identification of six separate types from a study of 37 subjects is extraordinary" (p. 240), and juxtaposits the skewed unimodal curve of individual differences of his subjects. But I never have even plotted my curve of individual differences both because in general a frequency type of a single curve of 37 items would be of little significance and because the particular conditions of the experiment were not the same for all subjects. (Had the curve been plotted it would be positively skewed, half-way between the knee-jerk and eyelid amplitude curves of Campbell.) What I did plot and fit were the magnitudes of the individual responses, and there I had 61 different curves with a total of about 6,000 items. Following Pearson and Elderton (**31, 10**), I found that 32 of my 61 curves were U-shaped or had $\beta_2 - \beta_1 < 1.8 > 1.0$ and that the magnitudes themselves fell into three distinct populations—or three types of responses—with ranges smaller than intermodal distances.[2] The 'six separate types' are not six types of subjects but six combinations of responses, combinations largely determined by chance. Thus, if the occurrence of a particular type of response from subject to subject and from conditioned trial to trial was completely a matter of chance, we would with 27 subjects and 3 trials have 7 types of combinations of responses in the subjects. 3 subjects would give in all 3 trials the same type of response, 6 would have different responses in each trial, and the remaining 18 subjects would in the 3 trials elicit 2 types of responses. Actually,

[2] Campbell reports a high amplitude reliability of first and last quarter *vs.* middle of 50 responses, but this can of course by no means take the place of curve analysis.

the incidence of types of responses in my subjects shows a greater cohesion, although it is in accordance with chance in that subjects with two-type responses—bi-modal distributions of responses—are the most frequent. In short, I found not 'subject types' but qualitative controlling factors in conditioning. The existence of such factors has been indicated before (**43, 5**) and their assumption has since proven valuable for other experimenters (**13, 14, 29**). I isolated these factors statistically and attempted, with some success, to correlate them with subjects' experience, experimenter's instruction, and situation set-up. My theory is, however, no more a theory of types than, let us say, a statement that *different* stimuli produce different *kinds* of imagery in *different* subjects makes the subjects 'imaginal types,' or still better than a statement that the *quantitative* ratings of people is influenced by the general *qualitative* affects—positive, negative, or indifferent—which each individual evokes in each rater, postulates a 'type theory' of rating.

4. Campbell misses apparently also my point on the relation of incidence to magnitude in human conditioning. He obtained a high positive correlation between these two criteria of conditioning and argues against my stating that they are separable and may result from the operation of two distinct factors. I, too, found fair correlations between magnitude and incidence with the *same* stimuli. But my statement is based upon results with *different* stimuli, having *known* differences in reactional biographies. The essence of my entire discussion was merely that the reactional biography of a conditioned or conditioning stimulus affects mostly the incidence and not the magnitude of conditioning, which remains more or less the same, varying only with physiologica lstrength. For instance, in my experiments words and nonsense syllables, which presumably differ in amount of past associations, also differed widely in the incidence—but not in the magnitude—of their conditioned salivation. On the other hand, eating pretzels and observing the eating of pretzels, which no doubt have similar reactional biographies, produced nearly equal incidence of conditioning, even though the magnitudes of their CRs were wide apart. Under the circumstances, the initial high correlations between magnitude and incidence of conditioning, observed in animal conditioning in which reaction histories are supposedly at minimum, become very much lowered in human subjects only when scores of different stimuli and responses are thrown together (**37**). However, with the same stimuli they appear often to stay fairly high, although it should be

mentioned that even here low correlations, as Campbell's own 0.26 for the eyelid response, have not been unknown (37).

5. Campbell again declares: "Such statements as Razran's that 'hundreds of human subjects' cannot be conditioned at all are obviously without justification. This would imply that each of these individuals had been subjected to all the conditioning procedures known to psychology and had proved negative to all of them' (p. 241). Campbell's second sentence is a misunderstanding of my statement and it is hard to see its import. My statement referred by no means to *possibilities* of conditioning but to actual results of past experiments. More particularly, it dealt with a comparison of adult human with animal and young children conditioning under the *same* normal laboratory conditions. Early in my interest in conditioning, in going over hundreds of detailed protocols of Russian laboratories, I was struck by the fact that, while dogs and young children were nearly always reported conditioned, numerous—fully hundreds—of adult human subjects failed. The differences could not be ascribed to differences in experimental setups since these are quite stereotyped in the Russian laboratories. I first heard that Pavlov so selected his dogs as to drop those that would not condition, but my stay in the Soviet Union and conversation with Pavlov and many of his assistants proved this rumor to be false. It is this difficult conditioning of adult human subjects in the *usual* set-up that first gave rise to my view of attitudinal control. The fact that these subjects may more or less readily be conditioned with certain procedures or responses is of course very much worth while investigating, but does scarcely remove the differentia between human and animal conditioning, which, furthermore, only naivité could attribute to differences in hypotheses, fields, or special tuning of stimuli and responses. At any rate, the statement about the conditionability of adult human subjects is a statement of fact and as such justified whatever its implications may be.

6. Campbell's last contention offers some grounds for argumentation and division of opinion, although even here the weight of present evidence is heavily against him. He writes: "Distributions of individual differences [in conditionability] . . . have been either normal or skewed but in *no case* have they been clearly bi-modal" (p. 226; italics mine). I, on the other hand, have maintained, on the basis of a review of the literature, that curves of individual differences in conditionability, while more or less normal and uni-

modal in animals and young children, tend to become U-shaped in older children and adults. Now, let me say at the start that as yet no *final* statement can be made about the shape of these curves for the simple reason that determining their shape or type requires a large number of subjects and strictly equal set-ups for all subjects in the same experiment, and past investigations have neither to offer. But tendencies have been marked and they will be considered. Campbell cites 6 experiments in which the curves of individual differences have clearly not been bi-modal: 2 on the eyelid by him and Bernstein (**2**), 2 on the knee-jerk by Schlosberg (**42**, **43**), one on the GSR by Darrow (**9**), and one on the finger withdrawal by Wolfle (**50**). (My own experiment might have been added.) Of these 6 experiments, the one by Wolfle can be practically disregarded here, since Wolfle not only used 12 different conditioning situations but also intentionally varied these from optimum to two directions of pessimum, and naturally some normalcy would be present. I plotted Schlosberg's data, which had unequal but not gradedly unequal conditions, but fail to see there any uni-modality. If anything, his results, lumping them all together, are sort of tri-modal, with a mode around zero, 50 percent, and 90–100 percent of conditioning; their β_1 and β_2 throw them off the chart of uni-modal or even U-shaped curves. Darrow's study does show normality but, as was said before, the GSR presents a special case since it is not consciously reportable and thus admittedly involves fewer attitudinal elements than all the responses under discussion here. There are left now the curves of the two eyelid experiments, with 31 and 63 subjects, which, although by no means normal, are unimodal, with a reversal of skewedness from initial to later stages of training.[3] Against these may be pitted the shock study of Osipova (**30**) with 327 children (142 normal) in which U-shapedness is clearly evident. True, the children of this experiment were not of the same age, but this is not a very disturbing circumstance since the youngest were seven years and particularly since, with the exception of the first two years, the U-shapedness is shown also on a small scale at each age level. I am also impressed by the fact that the distributions

[3] Campbell also argues against my stating that "it is entirely probable that, with further experimentation and assumedly more conditioning, the curve [Schlosberg's J-shaped of the initial conditioning of the knee-jerk] would become fully U-shaped" by this different transformation of the eyelid curve. In as much, however, as the two responses differ in some phases of their conditionability this *probable* prognosis still remains unrefuted, particularly since it gains some support from a comparison of the preliminary and the main experiments of Schlosberg's earlier data (**42**).

of Mateer with younger children (**28**) and of Osipova with subnormal children are, on the other hand, normal; that in the same experiment by Osipova 127 blind and deaf-mute children could hardly be conditioned, something which could much more readily be accounted for by attitudinal than by physiological factors; as well as by a host of smaller experiments on shock (**1**, **45**, **48**, etc.), saliva (**8**), and other responses in which extremes and 'yes-or-no' groupings were the rule. In general, the argument for the existence of qualitative factors in human conditioning from curves of individual differences rests not only upon the prevalence of U-shapedness in curves of adult human subjects but also upon the general non-normality of these curves and the fact that they become less and less normal with older and more intelligent subjects. Finally, it should be stated that while U-shaped curves presuppose attitudes, the converse is not necessarily true. In as much as my experiments indicate the preponderance of mixed rather than pure types of responses in individual subjects, i-shaped curves are not at all excluded.

Dr. Wickens' objections to my theory are much more general. Wickens found that subjects conditioned, through a combination of electric shock and buzzer, to make an extensor movement of the middle finger in a palm-down position showed a high degree of transfer when the hand was put in the palm-up position and the withdrawing movement was the flexor movement of the same finger, and vice versa. The attitudinal theory which, as he first admits, would fit his findings, is rejected by him on the following grounds. First, "When the subjects of this experiment were asked what they thought when the arm was turned over to the palm-up position following the extensor conditioning, the usual reply was that they didn't think of anything. . . . The finger just flew up without the subjects thinking about it . . ." (p. 118–119). Second, one subject could not extinguish his CR and even lost a bet to the experimenter. "The experimenter informed the subject that no more shocks would be given, and then even bet the subject a small amount that he could not inhibit his responses for the next ten stimulations of the buzzer. Much to the subject's disgust the experimenter won the bet" (p. 119). Third, "it would be impossible to put it [the theory] to a crucial test . . . it lacks . . . predictability . . . brings up the ticklish problem of determining what an attitude *is*, and it is doubtful that at the present state of our knowledge the phenomena of the conditioned response can be clarified by the introduction of an even more complex type of

behavior, unless it be that the conditioned response *is* complex because it must be subsumed under the *more* complex behavior" (p. 119 and footnote 4; italics Wickens').

Wickens' second objection will be considered first as the more specific and factual. Cases of subjects unable to inhibit or extinguish their consciously reportable CRs have been severally reported in the literature (**1**, **37**, **43**, **48**, etc.). They have, however, as in Wickens' own experiment, been singular exceptions. Easy inhibition and practically nil conditioning of consciously reportable responses in the face of subjects' antagonistic attitudes are of course the predominant rule. Schilder's results of 5 subjects who completely failed to be conditioned, despite 3–4 weeks of experimentation, may be cited as one of many examples (**41**). Another somewhat humorous example is that of Osipova (**30**) with a group of young communists who promptly inhibited all their CRs to shock when their leader, apparently catching on and indignant, authoritatively shouted: "Stop responding; she is fooling you." The explanation of the few exceptions to this rule, in terms of sets and attitudes, does not, as Wickens thinks, necessarily mean that ". . . we must introduce a prop and assume that the subject had a stronger unconscious attitude—granted there be such a thing—towards responding than conscious attitude against responding, and in this manner explain our results" (p. 119). A simpler account—in line with some conditionings in life-situations, phobias, sentiments, biases, and the like—might be that in certain instances the peripheral factors in conditioning, either little by little or even in one trial as in a number of emotional linkages, "come to control, instead of being controlled by, attitudinal sets and organizations" (**37**, p. 42). It should be stated, however, that there is nothing mysterious in subjects' attempts at attitudinal control producing reverse results. This has been the case with two of my subjects (**37**, p. 98) and is, to be sure, not unknown in experiments on sets and negativisms in suggestion. It also appears to me to be a plausible alternate hypothesis—the other being the just mentioned dominance of peripheral factors—for Wickens' exceptional subject Jo, who, unlike others, showed no immediate transfer of conditioning from a palm-down to a palm-up position, and vice versa.

Wickens' first and third objections can hardly be given any weight. He states that the questions 'what the subjects thought' were asked '15 or 20 minutes later' and that "the usual reply was that they didn't think of anything." Then, he asked them what

they thought "when their arm was turned over"—a reaction preliminary to the conditioning—and not 'when the buzzer was sounded,' the conditioning itself, as the attitudinal theory would demand. At any rate, to assume that the subjects had no attitudes because they did not report them 15 or 20 minutes later is too naïve, and the fact that the subjects reports were that "they didn't think of anything" virtually nullifies the entire value of the attempt. Nor can the last argument that the theory cannot be put to a crucial test, that it lacks predictability, and is too complex to be of aid in clarifying the phenomenon of conditioning, be taken very seriously. Subjects' introspections, experimenter's instructions, and above all special situational set-ups as preoccupation with another task, fatigue, emotion, correlation with age and intelligence, cumulation of CRs beyond conscious span, and the like, make attitudes in conditioning not only generally controllable but also specifically measurable and predictable (**38–40**). The complexity of the concept of attitudes also can hardly be held up as a deterrent. The history of psychology of the last twenty years has shown that oversimplification and 'reduction' have made little headway toward solving some very persistent psychological problems, and after all it is not so much the clarification of one or another isolated phenomenon as the interpretation of the dynamic totality of human behavior that is at stake. Finally, it must not be overlooked that attitudes bid fair to pave the way for a future integration of stimuli and motives, quantitative responding and qualitative tending, conditioning and testing, learning and personality.

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A REPLY TO DR. RAZRAN

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In as much as Dr. Razran (**8**) has credited me with misrepresentation, distortion, and several other unpleasant things it becomes necessary to consider each of his criticisms in detail. He has six specific objections and I treat them in his order.

The initial characterization of my study (**2**) as an unnecessary repetition of a thoroughly exploited problem is obviously not a very important part of Dr. Razran's thesis. However, I do find a certain interest in examining the studies which he apparently feels should have dissuaded me from my research. Razran's offhand thoughts are of course unknown to me and so are the untranslated Russian articles which he quotes frequently throughout his paper in support of his position. The translated abstracts to which he refers are in every case hardly more than indicative in their conclusions and in some instances have practically no pertinence to the problem at hand. The three studies which Razran cites specifically are presumably regarded by him as the most impressive of those which he mentions. In the first of these (Juschtschenko (**4**)) a total of *four* subjects was studied; in the second (Korotkin (**6**)) *nine* subjects were used, and in the third (Khozak (**5**)) there were *fourteen* subjects. To anyone who does not share Dr. Razran's enviable acquaintance with untranslated Russian sources 'all this' evidence is something less than conclusive.

1. Razran's first specific criticism arises from my statement as follows: "In considering the importance of attitude, it should not be overlooked that incidence of conditioning has been shown to be significantly related to at least two other factors, namely the sensitivity of the unconditioned reflex and the intensity of the conditioned stimulus. Razran's negative findings are distinctly atypical." This precaution I offered as a protest to Razran's apparent attempt to apply his theory of attitudinal control as an explanatory principle to the conditioning of 'any consciously reportable and consciously controllable response.'

Razran apparently agrees with me at one point in his argument that the importance of certain factors (unrelated to attitude) cannot be neglected. He then sets about attempting to prove that the first of the factors suggested by me, sensitivity of the uncondi-

tioned reflex on which the conditioning is based, is unimportant. Comparing his own findings to those of three Russian experimenters (all using regrettably few subjects) he concludes that in salivation ease of conditioning and reflex sensitivity are only negligibly related. The important point is that salivation is not representative of other reflexes which have been conditioned. The eyelid reflex has probably been the most thoroughly investigated for data of this kind. Three successive studies, one by Bernstein (1) with 101 subjects, one by Hilgard and myself (3) with 63 subjects, and my own recent study of 49 subjects have all demonstrated significant relationships between these two variables. The coefficients which I obtained in my study (2) of patellar conditioning are consistently positive and are entirely comparable to the relationship reported by Schlosberg (11) from similar data. Razran's mention of this latter study is incomplete and a partial misstatement. Since it has been clearly demonstrated that physiological factors unrelated to attitude as Razran has used the term are important in the determination of the conditioning of these two reflexes it seems obvious that in such cases his theory of attitudinal control cannot be unreservedly accepted.

In order to dispel any mistaken impression which may have been given that the results of Dr. Razran's study are in general representative of other research in the field of conditioning the following quotation from his study (9, page 40) should be cited: "As may have been noted, *the results* of the salivary conditioning of adult human subjects, presented so far in the study,[1] *differed markedly* from those of Pavlov on dogs, Krasnogorski on children, and in general *from any orthodox experiments on conditioning.*"

2. The second point revolves around the question of whether Razran meant types of response or types of subjects. I am quite aware that Dr. Razran has identified three kinds of conditioned salivary response, positive, negative, and indifferent. As I interpret his presentation, however, he has also given the very definite implication that subjects may be classified on the basis of which type (or types) of response they demonstrate. In several of his tables he has accompanied the number of each subject with a descriptive term 'positive, indifferent-negative, positive-negative, etc.' It is perfectly clear that Razran is not referring here to a positive-negative response (obviously an absurdity) but to a curve of responses characterizing each particular subject. In another instance (9, page 105) he refers to 'the differences . . . in the

[1] This description applies equally well to the later parts of Dr. Razran's study.

conditioning efficacies of different subjects' and this type of reference occurs repeatedly throughout his monograph.

Dr. Razran is annoyed at my 'confusion' in thinking that his results suggest that conditioning is a trait. If we examine his original study we find in the following quotation (9, page 104) that this was not only suggested but was clearly stated by him in so many words, "The possible relations between the conditioning 'types' and *other personality traits of the subjects* have not been studied and are unknown to the experimenter."

3. At this juncture Dr. Razran credits me with an 'apparent belief' which I must disown. My statement as to the exceptional nature of distinctly bi-modal or multi-modal distributions of conditioning scores does not refer to his plots of the magnitudes of individual responses given by the same subject. I made no reference whatever to the 32 U-shaped curves which he derived in this manner. To the contrary my comment referred to his dogmatic assertion (9, page 116) as follows: "The general conclusion that distributions of speed of conditioning tend to be . . . U-shaped and anti-modal in later ontogeny seems to be well warranted." The argument regarding the acceptability of this statement arises again at greater length in a later paragraph.

The matter of the 'six separate types' brings up again the question as to whether Razran actually divorced 'response types' from 'subject types' in his original report. The monotypes and bi-types of which he speaks therein are identified on the basis of the uniformity or disparity in the character (positive, negative, or indifferent) of the conditioned responses obtained from any single subject in a particular conditioning situation. Presumably the character of these responses depends on the attitude assumed by the subject. That is to say, the different kinds of distribution curves were determined by differences within the subjects, differences which were apparently relatively constant so long as the experimental conditions were unchanged. To quote Razran (9, page 104) at this point, "In the same experimental situation and with the same stimuli *the conditioning types of the subjects* seemed to have been more or less stable." There is then a more than subtle suggestion that there exist different types of attudinal set (or combinations of sets) possessed by the subjects and reflected in their behavior. Dr. Razran's direct identification of attitude types with subject types (as shown in distributions of ease of conditioning) is clearly expressed in the final lines of Point 6 in his article above (8).

4. Dr. Razran makes a great deal of what he terms my 'arguing against' his separation of the frequency and amplitude scores of ease of conditioning. My argument consisted of the following mild reservation: "Razran states that frequency and amplitude scores of conditioning result from the operation of two distinct factors and are unrelated. In this connection it should be noted that both in this study and in previous research these two scores have been found to be significantly correlated." Razran's finding that changing the nature of the conditioning stimulus produces a greater difference in the magnitude of the conditioned responses given than in their frequency is important and is the basis of his belief that these two scores are independently determined. The simple fact remains, however, that for any particular kind of conditioning the frequency and amplitude scores are commonly correlated to a significant degree. I have merely noted that in the consideration of Razran's results with differing situations the results of studies using constant situations are to be remembered.

5. On page 118 of Razran's monograph (9) this statement appears: "Nor do there seem to be cases on record where a normal child or animal could not be conditioned at all. There are of course hundreds of such adult human subjects." Dr. Razran is certainly correct in his objection that I have misunderstood him at this point. His explanation makes it clear that he did not refer to the impossibility of conditioning any particular subject in all conceivable conditioning procedures but to the imperviousness to conditioning occasionally shown by individual subjects within a single conditioning procedure. The truth of Dr. Razran's statement as correctly interpreted can be attested of course by anyone who has worked on the conditioning of adults.

6. Razran's final disagreement with my conclusions arises from a consideration of the nature of the distributions of individual differences in ease of conditioning. My original statement was as follows: "From consideration of the numerous distribution curves now in the literature it should be apparent that no typical distribution of conditioning scores can be assumed. The most consistent characteristic of curves obtained under diverse experimental conditions is the absence of conditioning types. Distributions showing distinct bi-modality or multi-modality are exceptional." Razran apparently shares my reservations concerning the drawing of final conclusions regarding the shape of these distribution curves but he is nevertheless attracted by the notion that a curve of U-

shaped quality is the most characteristic.[2] After a consideration of his argument and the data on which it is based I see no reason to change any part of my original contention.

It is clear that Razran's discussion of the six experiments cited by me does not bring to light any U-shaped distribution curves. His attempt to identify a 'sort of tri-modal' curve from consideration of Schlosberg's 49 subjects (12) conditioned in differing experimental situations is unimpressive indeed. His position seems to rest on the several studies of conditioned finger withdrawal-from-shock which he mentions. The most important of these is the study of Osipova (7) and even here we find that when the age factor is eliminated by considering separate year levels the U-shapedness disappears in some instances and is present only 'on a small scale' in the others. There is, moreover, a general criticism of the use of withdrawal-from-shock distributions in this connection. As I attempted to point out in my previous report it is hardly surprising if a distribution curve showing ease of conditioning turns out to be U-shaped if the distribution contains subjects who have no unconditioned response since no number of reinforced trials will produce a conditioned response in an unresponsive subject. Obviously this is apt to be the situation in the conditioning of withdrawal-from-shock as is very well illustrated by the study of Thorndike (13) in which two of the 53 subjects gave no withdrawal response to the shock whatever and another 18 of the subjects adapted to the point of non-response before the experiment was concluded. Clearly enough these subjects will comprise one arm of a U-shaped distribution. Those for whom the shock is an effective stimulus will condition to various degrees and their scores might be interpreted as forming the other arm of the distribution. However, such a spurious bi-modality can scarcely be offered as validation of Razran's theory of attitudinal control of conditioning.[3]

Dr. Razran's footnote at this point refers to his prediction that

[2] It should be noted that Dr. Razran is much less positive on this point in his present article than in his original monograph wherein he unreservedly stated, "The curves of individual differences in speeds of conditioning are more or less normal in phylogeny and in early ontogeny, but become U-shaped in later ontogeny."

[3] In this connection it is interesting to observe that in the extensive review (10) of withdrawal-from-shock literature written by Dr. Razran in 1934 he concluded with the following statement: "While the withdrawal response to an electric shock is undoubtedly most useful in animal conditioning and of great interest in some special problems of human conditioning, its general value for the discovery of the laws of conditioning in adult human beings is extremely limited."

Schlosberg's J-shaped curve of patellar conditioning would have become U-shaped had the conditioning series been extended. From an analysis of some of my data it was possible to show (3) very clearly that such a prediction would have been completely erroneous if applied to eyelid conditioning. Razran objects that eyelid responses and knee-jerks are not the same and reaffirms his confidence in what he considers to be a 'probable prognosis.' This seems to be a matter of faith. In the face of the contrary evidence which he himself cites and in the light of the ambiguity of his supporting evidence, Razran's continued insistence on the 'prevalence of U-shapedness in curves of adult human subjects' appeals to me as lacking in experimental justification.

With this brief consideration of each of Dr. Razran's objections to my original comments regarding his monograph and his theoretical position, I find no reason to alter my original thesis. To my mind, Dr. Razran has applied his hypothesis of attitudinal control too broadly. Not all 'consciously reportable and consciously controllable responses' condition in the manner of his description. The experimental results of the study from which Dr. Razran was led to his theory of attitudes are by his own description unorthodox rather than typical of the findings usually obtained in this field. The explanation which is quite satisfactory for Razran's results is not well suited to the results of experiments such as my own. In conclusion may I point out that I have at no time denied the significance which 'attitudes' may have in certain instances in the determination of conditioning scores. The essence of my criticism of Dr. Razran's position is contained in the following statement from my previous article: "There can be no doubt that the subject's attitude is an important consideration in conditioning experiments. It is apparent, however, that in certain conditioning procedures there are also other factors operating, the significance of which cannot be summarily dismissed."

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A REPLY TO DR. RAZRAN'S "CONDITIONING AND ATTITUDES"

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In replying to Dr. Razran's criticism, the writer would like to state in the beginning that he believes with Razran and others that the CR is influenced by the subject's attitude; but he disagrees with the former in regard to the relative influence of these attitudes. Thus, he does not hold with Razran (3) that "the attitudes are dominant over the mere linkages of the situations presented," and that they have the 'ability to prevent and stop CR formations that are incongruous with the individual's set of symbolic reactions, or ability to symbolically nullify the action of conditioning stimuli.' Also he does not believe that consciously reportable and consciously controllable responses require new laws and generalizations.

To this difference of opinion Razran's criticism of what he calls my second objection is particularly pertinent. He states that subjects who are unable to inhibit or extinguish their consciously reportable CR's are, although mentioned in the literature, only

'singular exceptions.' The writer would like to mention several recent studies which have specifically attacked this problem and arrived at a somewhat different conclusion.

Hilden (1), working with finger retraction from shock, and using action currents from the muscles involved in making the withdrawal movement, as well as overt finger movements, instructed a few subjects to fight against the development of the CR. But, despite the acceptance of this attitude, he found that these subjects did become conditioned. Furthermore, his delicate measures often indicated that CR's were present *even though the subject was not aware that he was making these responses.* Here we have the strange picture of a consciously reportable response going on below the level of consciousness. As a result of the study Hilden draws the conclusion "that the response was involuntary, that it could not be inhibited, and that responses occurred often without awareness of their occurrence on the part of S." What a far cry this is from Razran's consciously reportable and consciously controllable response!

A slightly different approach, using the same response but recording only overt finger movements, has been made by the writer (7). After an arbitrary number of presentations of the shock and the sound of the buzzer together, the subjects were instructed that from now on they were not to respond to the buzzer unless or until the shock occurred. Even so, it was found that most of the subjects were unable to inhibit completely responses to the buzzer before presentation of the shock.

Again the work of Hilgard and Humphreys (2) on a similar problem is of interest. After developing a simple conditioned eyelid response to light with an air puff to the cornea as the conditioned stimulus, the process of developing conditioned discriminatory reactions was begun. Before attempting the discrimination the subjects were divided into four groups, to each of which a different attitude was suggested. Although the entire study is highly pertinent to the problem under discussion, it is sufficient for our immediate purposes to consider only two of their groups, namely the control group which was given no instructions by the experimenters, and the experimental group which was requested not to respond to either the positive or negative stimulus. Considering only the percentages of responses to the positive stimulus, we find them to be 74 for the control group and 55 for the above mentioned experimental group.

The results of these experiments, then, are all very much in accord in indicating that antagonistic attitudes are not dominant over mere linkages of the situations presented. The first study indicates that a negative attitude may not prevent the formation of an association between the incidental stimulus and the movement produced by the unconditioned stimulus, and the latter two studies show that, once the connection has been established, a negative attitude may decrease the frequency of occurrence of the response, but it does not reduce it to zero.

As a contrast to these sorts of results, Dr. Razran has mentioned the studies of Schilder and of Osipova. The writer, however, feels that there are several legitimate objections to the use of Schilder's (5) study as evidence for the dominant influence of attitudes. To begin with, the long interval of five seconds between the conditioned and the unconditioned stimulus was used. No mechanical recording was employed, and the experimenter did not seem to consider anticipatory movements as CR's. But, despite these inadequacies, the experimenter himself reported that three of his five subjects did give CR's; and the writer feels that the fourth subject who made slight movements of the finger in anticipation of the shock should bring the total number of subjects conditioned to four. Although it is granted that the conditioning in this experiment was somewhat sporadic, it can hardly be said that all of these subjects 'completely failed to be conditioned.'

The writer feels somewhat as Razran evidently does, namely, that his second criticism is the most significant, and there are only a few comments that he wishes to make on the discussion of his first and third objection. Razran states that it is naive to assume that no attitudes were present because the subjects could not verbalize them 15 or 20 minutes later, and I agree. This fact was mentioned in the previous study, not to imply that no attitudes were present, but only to indicate that, if they were, it was exceedingly difficult to isolate them by means of verbal reports. In regard to my objection concerning the difficulty of controlling attitudes, I still feel this to be a knotty problem, but I have already admitted in a footnote to my previous article (6) that Razran (4) has made an important step in this direction.

In conclusion, the writer wishes to state that he certainly has not meant to deny the significance of attitudes in conditioning any more than he would deny the influence of them in conventional learning set-ups. He feels, however, that, although they may

facilitate or retard the formation of connections, they serve only these purposes, and that something more is involved in the establishment of the connections themselves.

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